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ABSTRACT

This document contains the 1994 edition of a publication designed to share information in the field of linguistics that is of practical, theoretical, administrative, or general interest. Articles in these issues include: "Ethnography Vs. Questionnaire" (Thomas E. Murray); "Differences: A Diary Entry"--concerning structuralism and deconstruction (John Verhaar); "Reflections on Isthmus Zapotec Inflection" (David Weber); "The Category 'Irrealis' in Papuan Medial Verbs" (John R. Roberts); "Language Underived: A Diary Entry" (John Verhaar); "Validity and Reliability in Language Survey Testing" (Barbara F. Grimes); "Recent Developments in CECIL"--concerning an accoustic software and hardware package (Geoffrey Hunt); "On the Motivation for Feature Geometry"--on a branch of phonology (Steve Parker); and "Mapping Semantic Relationships in the Lexicon Using Lexical Functions" (Charles E. Grimes). Each issue is prefaced by comments by David Payne, and each contains, in addition to the articles, some or all of the following features: "Remarks and Replies"; "Reports"; "Reviews"; and "Announcements." (LR)

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Coordinator's Corner

This is the first issue of *Notes on Linguistics* for which I am serving as editor, though the articles, reports, and reviews included in this issue were accepted and initially processed by my predecessor in this position, Eugene Loos. As a tribute to Eugene, Winfred Lehmann, one of SIL's International Linguistics Advisors, has kindly permitted me to cite from a letter he addressed to this office around the time of our transition. Dr. Lehmann wrote the following to Eugene:

> For some time I have been wanting to thank you for your useful journal, and now I'd better get about it since you seem to be taking off for other work. It has been a pleasure to read the articles, reports and reviews, especially because they are based on information the authors clearly control, and at the same time presented in a theoretically unbiased form. Moreover, the authors do not go on and on, but give their findings crisply. I'm grateful to them, and especially to you who have managed to keep the journal on such a high level.

I, too, will take this opportunity to thank Eugene for the years dedicated to this work. And for those of you who have contributed crisp articles, reviews and reports, please keep them coming.

By way of other news, in December the Linguistics Department, together with the Academic Computing Department and Project '95, sponsored a working seminar on 'Conceptual modelling in phonology'. Results included the basic framework (a formal conceptual model) on which phonology applications in CELLAR (Computing Environment for Linguistic, Literacy, and Anthropological Research) are being developed. Mike Maxwell will carry this project forward.

Another meeting which took place in December was of a newly formed Guidance Team for CADA/CARLA/AMPLE/STAMP, a group of related computer programs for doing morphological parsing and adapting literature between dialects or closely related languages. The team consists of Andy Black, Alan and Karen Buseman, David Payne (Linguistics Coordinator), and Gary Simons (Academic Computing Coordinator). This team did the groundwork for processing changes to and future developments in these programs.

—David Payne

Ethnography vs. Questionnaire: Investigating Lexical Choice in the Language of St. Louis, Missouri

E. Thomas Murray
Kansas State University

1. In 1986, I published a dialectological and sociolinguistic study titled *The Language of St. Louis, Missouri: Variation in the Gateway City*. In completing the field research for that book, I combined some of the methods of two very different traditions: I collected the phonological data ethnographically, but used a more traditional questionnaire in gathering the morphological/syntactic and lexical data (for a more complete description of the methods, see Appendix 1). This blend of techniques was necessary, I believed, because

> ...after securing my informants... and beginning my interviews, it became apparent that although traditional techniques would suffice more than adequately for the collection of data on morphology, syntax, and lexicon, they were not at all suitable for the procurement of phonological data. Not only were my informants extremely uneasy when they became aware that I was observing their pronunciation, they quite obviously tried to effect certain changes in the formation of many of their vowels and consonants. ...Even my attempts to lure them into a number of different stylistic shifts proved fruitless: neither the well-known "danger of death" question...nor any other technique could diminish their phonological self-consciousness (1986:6).

I also wrote at the time, however, that

> ...some may criticize this eclectic union, but the vast majority of researchers have come to realize that the goals and needs of their work are frequently met far more satisfactorily by combining the best aspects of two or more fundamentally different paradigms than they would have been through a strict allegiance to only one of them. In the following discussion, then, I will merely describe the major procedures I followed in the present study...and assume a thorough defense of them to be unnecessary (1986:5-6).

My belief that '...some may criticize this eclectic union' was prescient; my assumption that 'a thorough defense' of it was 'unnecessary', however, proved erroneous. Indeed, Guy Bailey opined

in his 1988 review that I had not adequately addressed the issue of how that 'amalgamation of methods' had affected my data, and implied that morphological/syntactic and lexical data collected ethnographically might well have yielded different results than I obtained using traditional survey methods. More precisely, he wrote:

> ...Murray says too little about how he has collected his data and the impact of his methods on his results. For example, Murray notes that his procedures combine the methods of dialect geography and "sociolinguistics as practiced by a growing number of ethnographic participant-observers" but says little about how the amalgamation of methods affects his data. ...he suggests that this methodology is most important for getting at phonological differences, [but] he does not illustrate explicitly the kinds of differences that emerge.
>
> Readers may be quite surprised at Murray's claim that [ethnographic methods] are required not for grammatical features (which he says can be elicited adequately through traditional means) but for phonological ones since many nonstandard grammatical features occur almost exclusively in casual contexts. Again, he does not give specific examples so it is hard to evaluate his claim, but in the light of its uniqueness, it deserves greater attention (1988:67).

Although I attempted to address these criticisms elsewhere (1988:136-37, 139-41), this is the first opportunity I have had to provide the full amount of 'greater attention' Bailey called for. In this essay I would like to take up that very issue of methods and results—specifically with regard to the language used in St. Louis, Missouri, but with possible implications for data collected in any speech community: Do Gateway City speakers make different lexical choices in **unguarded** speech than they claim to make when answering direct questions about their usage? If so, what governs such choices? If not, why? Most importantly, perhaps, do the results of this current analysis yield any new information regarding linguistic field methods and their reliability?

2. Arriving at an empirical answer to the first question posed above—whether St. Louisans make different lexical choices when they speak than they report making—seems fairly straightforward: One need merely compare my informants' **guarded** answers on the questionnaire portion of the survey with their **unguarded** choices on the ethnographically recorded tapes (which, in addition to phonological data, inadvertently collected a great deal of lexical data

as well), then tally the results. As is often the case, there is more here than meets the eye. I recorded nearly 1500 hours' worth of cassette tapes for my original project on the language of St. Louis; and while I am not the first to note that every hour of recording demands several more of listening, I will observe that the hours spent listening multiply exponentially when one is searching for any of 110 morphological/syntactic items (Appendix 2) or 96 lexicon items (Appendix 3)—the numbers of items on my St. Louis questionnaire. In fact, I most often had to proceed sentence by sentence, first listening, then cross-referencing with my checksheets of items, then noting the relevant demographic characteristics of the informant speaking (24 demographic cells of informants were represented; Appendices 4 and 5). The task was, simply put, gargantuan, and I cannot apologize for taking nearly a decade to complete it. It began late in 1982, as the original study was still proceeding, and gave only a preliminary assessment of my findings in a response to Bailey (1988:141); Bailey's criticism, in other words, merely provided a further impetus to complete a task that until then had been motivated solely by my own curiosity.

However, even such a vast source of data proved insufficient for my comparative needs. I had decided early in the current project to discount a demographic cell from the final comparison if I could not place at least 10 responses in it (with the exception of the cells for *a + participle* and *might could*, neither of which my original survey had found to occur in the language of St. Louis).[2] All told, that requirement forced me to eliminate 483 cells of morphological/syntactic responses (18.3 percent of the total) and 1113 cells of lexicon responses (48.3 percent of the total) from consideration. The remaining cells—2157 containing morphological/syntactic responses and 1191 containing lexicon responses—could then be compared with the data from the original survey. In performing that comparison, I first converted all raw scores into percentages, then used the Pearson product-moment coefficient test of statistical significance to determine how well the two sets of data (morphological/syntactic: questionnaire vs. ethnographic; and lexicon: questionnaire vs. ethnographic) correlated for each of the 24 demographic cells.[3] The results of those tests are presented in Appendix 5.

As Appendix 5 illustrates, the coefficients of all 24 demographic cells are above +0.80 (ranging as high as +0.91) for the lexicon data, and

+0.70 or above (ranging as high as +0.83) for the morphological/syntactic data, indicating a very high level of correlation for all subgroups of informants with regard to both groups of data. Interestingly, there appears to be no pattern to the coefficients regarding specific demographic variables for either set of data. In other words, regardless of whether we examine the morphological/syntactic coefficients alone, the lexicon coefficients alone, or both sets of coefficients together, it is not the case that stronger or weaker correlations consistently exist for any demographically-defined group of people—whether that group is defined by class, gender, age, or some combination of these variables—than for the others. The answer to the first question posed above, then, is negative: by and large, St. Louisans do not make different morphological/syntactic and lexicon choices in unguarded speech than they claim to make when answering direct questions concerning usage; or, put another way, St. Louisans **believe** they use their verbs, particles, prepositions, and lexicon approximately as they **do** use them.

3. This presents an interesting—or, as Bailey (1988:67) observed, relatively **unique**—state of affairs, and especially with regard to the two sets of morphological data. It has long been axiomatic in studies of language variation, especially those done in the United States, that verb usages elicited by questionnaire are not to be entirely trusted. Modern speakers of American English have inherited a 300-year legacy of **rights** and **wrongs**, which legacy continues to be inculcated almost from birth by well-intentioned parents, siblings, teachers, and society in general. The resulting paranoia is frequently so strong that many if not most of these speakers will go to great lengths to conceal their perceived **errors** from anyone believed to be in a position to judge—particularly from a stranger collecting data on how people conjugate their verbs. In short, Bailey was right to be somewhat suspicious of my statement in the original survey that the questionnaire method 'would suffice more than adequately for the collection of data on morphology...' (1986:6).

How, then, to explain the coefficients shown in Appendix 5? At the conclusion of my 1986 study, I constructed an elaborate subjective reaction test to determine precisely why St. Louisans have the attitudes they do toward their speech (1986:217). The test consisted of randomly-ordered, 30-second tape recordings representing speakers

from both genders, two races (black and white), and three socioeconomic classes (upper, middle, and lower). The speakers were all people who had been eliminated from consideration for use in the original study; the recordings typically consisted of answers to queries concerning food items, childhood games, or some other aspect of life in St. Louis, and did not attempt to isolate the speakers' phonology, morphology/syntax, or lexicon. All the respondents to the questionnaire portion of the survey listened to each of the tapes, then answered these two questions:

(1) If you had to characterize this person [the speaker on the tape] in a word or two, what would the one or two words be, and would you consider that characterization positive or negative?

(2) If you had to guess, where would you say this person was probably born and raised, and would you consider that place positive or negative?

The overwhelming negative response to the first question involved the epithet *Hoosier* (see 1986:253; 1987); and '[more than] half of the respondents who judged a speaker negatively believed that person to have been born and raised in the Ozarks region of southern Missouri' (1986:253). These results led me to the following conclusion:

> There is clearly a correlation in the minds of these judges between the **nonstandard** linguistic habits of a **Hoosier** and the stereotypical Ozarkian speaker. And we must note that it does not matter whether Ozarkian speakers **actually** have any speech traits in common with St. Louis Hoosiers; what is salient here is that Gateway City residents **perceive** the two groups as sharing a similar if not identical language.
>
> Surely settlement and migration patterns and the city's status as an urban center helped to determine linguistic usage initially; perhaps these factors still play a role in St. Louisans' speech patterns today. But there is also an undeniable psycho-social component to language choice in St. Louis: Speakers may often choose 'northern-sounding' forms because they wish to dissociate with the Ozarkian South, the residents of which serve as the stereotypical Hoosiers that call to mind such negative images in their city-dwelling counterparts (1986:253).

Now back to the question posed earlier: Why are St. Louisans so comparatively unselfconscious about their morphology, even to the point of answering direct questions about their usage more or less honestly? I believe that the strongly negative reaction that so many

respondents had to the subjective reaction test in 1986 was primarily a reaction to the 'nonstandard' pronunciation patterns they perceived. St. Louisans are relatively more willing to endure (and engage in) a little 'bad grammar' than they are to endure (and engage in) the kinds of phonological patterns that they believe are reflective of an Ozarkian mentality. In many other places in the United States, speakers may desire more than anything else to avoid such solecisms as subject/verb disagreement, *ain't*, and multiple negation; but in St. Louis, the overriding fear involves pronouncing one's words like a Hoosier.

4. I have, of course, saved the most intriguing and most difficult question for last: Do the results of the analysis presented in this paper yield any new insights regarding linguistic field methods and their reliability? Unfortunately, the most authoritative answer I can muster is 'perhaps'. I believe Bailey's doubts regarding my 1986 field methods were well-founded. That I attempted to justify them by citing my preliminary findings and, to a lesser extent, my status as a native St. Louisan, could count for only so much. And that I have now justified them statistically should certainly not be construed as an inductive window through which other field workers gathering data in other places may pass. As I stated just above, I believe St. Louis to be different from many other locations in the United States: in that city (but in how many others?), it is phonological rather than grammatical taboos that induce great linguistic insecurity and even paranoia.[4] The point, in other words, is that 'conventional wisdom' is usually considered as such for a good reason—because empiricism has proven it true. We should not allow such wisdom to dictate our every methodological move, certainly, but it is only right to continue believing in it until we have valid proof of some reasonable alternative. I have provided that proof for one such alternative in one speech community; it remains to be seen whether the same alternative is viable in other speech communities as well.

Notes

1 An earlier version of his paper was read at the annual midwestern meeting of the American Dialect Society, November 1992, in St. Louis, Missouri.

2 I selected the number 10 largely at random, though I was guided in part by the suggestion of Wolfram and Fasold (1974:39).

3 The Pearson product-moment coefficient (*r*) produces correlations that can range from -1.00, representing a perfect negative correlation, to +1.00, representing a perfect positive correlation, with 0.00 representing no correlation at all. Although there is no accepted minimal level for a particular correlation to be **significant**, of course the closer the *r*-value is to +1.00, the stronger the correlation is.

4 This observation does not deny the reality—first shown so clearly by Labov (1963, 1972), and since reinforced by dozens of other researchers—that the members of any given speech community typically use phonological markers to make judgments about, categorize, and even stereotype one another. In the vast majority of these communities, however, it is probably still true that the prevalent phonological markers are less stigmatized than such grammatical markers as *ain't* and double negation.

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Appendix 1
Synopsis of Methods Used in Murray (1986)

Method 1 (questionnaire), used for morphological/syntactic and lexical data

I followed closely the procedures for eliciting data used by the fieldworkers of the Linguistic Atlas: suitable and willing informants were located, then data were collected slowly and systematically through the use of questionnaires and personal interviews. For example, the first item on the morphology/syntax questionnaire was **BITE (past part.)**, with the sample elicitation sentence being 'The dog has _____ the mailman again' (to the informant: 'How would you fill in the blank in this sentence using some form of the verb *bite*?'); and the first item on the lexicon questionnaire was A SMALL, ROUND MELON WITH ORANGE FRUIT AND A ROUGH RIND (to the informant: 'What do you call. . . ?'). All questions and answers were given orally, and the informants' responses were recorded by hand and later tabulated.

Method 2 (ethnography), used for phonological data

Here I resorted to the use of surreptitious recording (for the legality and ethics of such procedures, see Murray and Murray 1992). After using preliminary interviews (as well as my own preconceptions as a native St. Louisan) to isolate the phonological variables that St. Louisans were especially wary of producing, I followed these steps:

(1) I located contexts in which one or more of the phonological variables was produced frequently and by a wide range of people (see (3) below), then blended into and became a part of those contexts. I therefore collected data in such diverse places as singles bars, funeral parlors, grocery stores, churches, laundromats, restaurants, gas stations, playgrounds, and, in short, any public place where I could work inconspicuously and with the permission of the establishment's proprietor.

(2) I determined three general levels of linguistic formality—informal, midformal, and formal—by noting such characteristics as whether the language was being produced for the first time or was being repeated (following Labov, 1972b, 43-69, I counted multiple repetitions of the same language with different levels of emphasis as being at different levels of formality), the context of the language (casual encounter, public speech, etc.), the purpose of the language (to inform, to teach, to socialize, etc.), and so forth. All judgments, however subjective, were my own, and so were consistent.

(3) I classified informants demographically according to their age (approximated, but confirmed through random sampling to be accurate more than 90 percent of the time), gender (also, in some cases, approximated), social class (based largely on the location of the context being investigated, again following Labov's technique in his well-known department store survey, 1972b:43-69), and status as native St. Louisans (also approximated, but once again confirmed through random sampling

to be accurate more than 95 percent of the time). All doubtful cases were discarded in the final analysis.

(4) The tools I used for recording were a pocket tape recorder and, when background noise proved prohibitive, a small pad and pencil.

Appendix 2:
110 Morphological/Syntactic Forms Queried in Murray (1986)

bite (pst.prt.)	blow (pret.)	break (pst.prt.)	burst (pret.)
buy (pst.prt.)	catch (pret.)	climb (pret.)	come (pret.)
dive (pret.)	do (pret.)	drag (pret.)	draw (pret.)
dream (pret.)	drink (pret.)	drink (pst.prt.)	drive (pret.)
drive (pst.prt.)	drown (pst.prt.)	eat (pret.)	eat (pst.prt.)
fight (pret.)	fit (pret.)	give (pret.)	grow (pret.)
grow (pst.prt.)	hang (pst.prt.) ('the murderer was __')	hear (pst.prt.)	heat (pst.part.)
help (pret.)	help (pst.prt.)	kneel (pret.)	knit (pret.)
know (pret.)	lend (pret.)	lie (pres. inf.)	lie (pret.)
plead (pret.)	ride (pst.prt.)	ring (pret.)	rise (pret.)
run (pret.)	see (pret.)	set (pres.)	set (pret.)
sew (pst.prt.)	shrink (pret.)	sit (pres.)	sit (pret.)
sneak (pret.)	spoil (pst.prt.)	steal (pret.)	sweat (pret.)
swell (pret.)	swell (pst.prt.)	swim (pret.)	take (pret.)
take (pst.prt.)	teach (pret.)	tear (pret.)	throw (pret.)
wake (pret.)	wear (pst.prt.)	write (pst.prt.)	be (1 p.s.)
be (3 p.p.)	cost (3 p.s.)	do (3 p.s.)	have (1 p.s.)
have (1 p.s.) + be	make (3 p.s.)	rinse (3 p.s.)	say (1 p.s.)
work (1 p.s.)	work (1 p.p.)	be ('cabbages + be')	be ('here [contracted be] your clothes')
be ('oats + be')	be (pres.) ('there [be] many people')	be (2 p.p.)	be (1 p.m. pret.)
say (3 p.p.)	think (3 p.p.)	be (1 p.s.) + not	be (1 p.s. + NOT)
be (3 p.s.) + not	dare (1 p.s.) + not	have (1 p.s.) + not	ought (3 p.s.) + not
used to (3 p.s.) + not	a- + pres. part.	go (pres. part.)	rot (prs.prt.)
tell (inf.)	might could	wants in	wants off
wants out	all ___ once	buttons ___ the coat	half ___ six
___ home	I'm not (___) sure	name a child ___ somebody	quarter ___ the hour
___ sick	sick ___ the stomach	stand ___ Elm St.	stand ___ line
wait ___ someone			wood ___ the stove

Appendix 3: 96 Lexical Forms Queried in Murray (1986)
(descriptions have been abbreviated and occasionally changed to save space)

melon: orange fruit, tough rind
flavored cake topping
thick white food w/ curds
flat breakfast food, served with butter and syrup
large sandwich w/cheese, meats, etc.
center of cherry
remove bean/pea coverings
small, edible fresh-water crustacean
insect: often glows in dark
small, black, odorous animal w/white stripe down back
high level of water vapor in air
dog of uncertain lineage
male sheep
piece of furniture: stores clothes
metal container: used to carry water
web made by a spider
dust that collects under bed
metal container: used to prepare food on stove
bathroom located outdoors
kitchen appliance: keeps food cold
paper container for groceries, etc.
term of address for male parent
neutral term for black person
members of extended family
water source on street (used by fire-fighters)
trucks loaded, unloaded here
expel gas through mouth
for child, parent to share physical features in common
nurture a child to adulthood
when road is wet/icy
when car receives lubrication
what men wear for swimming
used by children on snowy hills
apparition
what dead person is buried in
store-bought fruit spread
corn served intact on cob
round pastry w/ hole in center
food eaten between meals
kind of long bean
center of peach
machine: dispenses cold drinking water
large insect: found near fresh water
very small fish: often used as bait
very small, fresh-water river
kind of maple tree
where pigs are kept
sound a horse makes
built in wall: clothes hang here
wood container: used to carry water
indoor water receptacle
large room: used for entertaining, relaxing
part of house: extends beyond front door
piece of furniture: several people sit on it together
indoor receptacle for refuse
term of address: female parent
pejorative term: black person
term of address: mixed group of males and females
playground equipment: used by two children at once
ride downhill on sled, on stomach
when something is on diagonal plane
when baby moves on all fours
child purposely misses school
magnifying glasses used at sporting events, etc.
small mouth instrument often played at campfires
far away/far to go
homemade fruit spread
yellow bread: made w/corn
made w/ice cream/soda: eaten w/ spoon/straw
carbonated beverage
bone in breasts of fowl
coverings on ear of corn

small, brown woodlands animal: stripe down back
long, slimy creature: often used as bait
very large worm: often hunted during late evening
sudden, large rain
male cow
sound a cow makes
covering for windows: can be raised/lowered
guides smoke from fireplace to outdoors
dust/straighten up the house
web of uncertain origins
outdoor water receptacle
basement room where vegetables/fruits are stores
supports wood during cutting
outdoor receptacle for refuse
pejorative term: white person
neutral term: member of police force
where fire engines are stored
largest over-the-road truck
when one dives into water, lands on stomach
go with someone, as on a date
when an adult babysits a child
what baby is wheeled around neighborhood in
channels water off of roof

Appendix 4: Demographic Cells Used in both Murray (1986) and the Present Study
(adapted from Murray, 1986:11)

class: upper/middle/lower	
gender: male	gender: female
age: < 20	age: < 20
age: 20-40	age: 20-40
age: 40-60	age: 40-60
age: 60-80	age: 60-80

Appendix 5: Statistical Correlations between Ethnographic and Questionnaire Data

Demographic Cell	Morphological/ Syntactic Data	Lexical Data
upper-class males, aged < 20	r = +0.70	r = +0.84
upper-class males, aged 20-40	r = +0.72	r = +0.87
upper-class males, aged 40-60	r = +0.75	r = +0.82
upper-class males, aged 60-80	r = +0.75	r = +0.90
upper-class females, aged < 20	r = +0.73	r = +0.85
upper-class females, aged 20-40	r = +0.80	r = +0.84
upper-class females, aged 40-60	r = +0.79	r = +0.83
upper-class females, aged 60-80	r = +0.71	r = +0.88
middle-class males, aged < 20	r = +0.74	r = +0.89
middle-class males, aged 20-40	r = +0.78	r = +0.83
middle-class males, aged 40-60	r = +0.72	r = +0.86
middle-class males, aged 60-80	r = +0.83	r = +0.82
middle-class females, aged < 20	r = +0.71	r = +0.85
middle-class females, aged 20-40	r = +0.77	r = +0.91
middle-class females, aged 40-60	r = +0.82	r = +0.85
middle-class females, aged 60-80	r = +0.76	r = +0.87
lower-class males, aged < 20	r = +0.75	r = +0.81
lower-class males, aged 20-40	r = +0.72	r = +0.89
lower-class males, aged 40-60	r = +0.80	r = +0.82
lower-class males, aged 60-80	r = +0.73	r = +0.88
lower-class females, aged < 20	r = +0.70	r = +0.84
lower-class females, aged 20-40	r = +0.76	r = +0.81
lower-class females, aged 40-60	r = +0.71	r = +0.86
lower-class females, aged 60-80	r = +0.79	r = +0.83

■

Differences: A Diary Entry

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As a student, in days of yore, I imbibed 'structuralism', and other Saussurean mysteries. It all seemed rather implausible. The 'linguistic sign' was 'arbitrary', I learned. I asked the instructor if that view could emerge at all except if you pretended you didn't know the languages you knew. The response brushed my scruples aside by insisting on the need to be 'scientific'. Daunted, I blinked first. Also I tried to be convinced of the 'form-not-substance' principle. Language, I was told, was a sum total of innumerable differences. Differences between what and what? Well, there weren't any such 'whats'—not as 'things', anyway, but rather as 'points', as in math—dimensionless. This was the 'diacritical' view of language.

It was hard to get used to that view but I pretty much had to when digesting Bloomfieldian things like parallel and complementary distribution. Later I learned about 'distinctive features', and 'componential analysis'—especially in studies of kinship terms. And, yes, all that was 'diacritical'. After all, how could anyone be a 'son' without being the son of so-and-so, and similarly with daughters, aunts, in-laws—that was all clearly 'relational', amounting to forms, and forms of forms. It wasn't just the case of kinship terms, of course, but also colors, 'elevationals' (those 'up trains' and 'down trains', for example; or that future 'down the road', and thus a future 'lower' than now), and sundry other things. I began to be anti-structuralist only now and then, such as when someone said 'structuralism!' at times when I didn't want to blink first.

Time came when I struck out into other social sciences: Lévi-Strauss, in anthropology, based himself on Saussure. So did Jacques Lacan in psychology; and Althusser in history; and Merleau-Ponty in philosophy—by that time I had dived into philosophy, and was, for a while, an ardent phenomenologist (Ken Pike used to draw me out on that one). But Merleau-Ponty was the first, perhaps unbeknownst to himself, to apostatize from phenomenology and to become a

'structuralist'—a change of heart (rather than of mind, I felt) that came over him after reading Saussure. Later, 'structuralism' apostatized from itself and it has now become 'deconstruction'—everywhere except in linguistics.

What, if anything, is 'deconstruction'? Let me try to explain. Deconstruction works with 'texts'. Texts lead their own lives. A text (literary or philosophical or, of course, linguistic—such differences don't make any difference) proposes some claim, or thesis (T). But any claim may be made in any of many ways (W). After all, any text is based on prior text. Now, here's an article of the deconstructivist's creed: In any text, T and W always clash. The author not only can't help this but is even normally unaware of it. Hence texts have no authors. There's no such entity as an 'author', for he/she has not produced the text: 'it' has produced the 'author'. We use phrases and words which we have received from others—thus de Saussure's claim amounting to the form-not-substance principle was not created by de Saussure—'he' was created by that claim, dragging along with it a few millennia of (undeconstructed) conceptions of 'form' and 'substance', and of the networks defining them. And so, yes, I admit, I am not producing this text; rather, this text produces me—at least for today.

* * *

Deconstruction is the natural outcome of structuralism. De Saussure almost certainly never realized what he started. The start itself was as clear as it was original. It doesn't matter what form language (L) has, provided the form it has isn't seen as something it 'has'; rather, that particular network of relations that L 'has' is what L 'is'. The arbitrariness thesis must have been a by-product of some prior text of his text—for 'arbitrariness' can be understood only as parasitic upon the understanding of what is 'necessary', or 'natural', or 'motivated': in short, of some 'substance'. To say that the 'linguistic sign' is 'arbitrary' is to deny rather than to assert something. But denials have assertive prior texts. De Saussure promulgating the arbitrariness thesis reminds me of my five year old nephew who once approached me with his hands behind his back saying, "Uncle, I have nothing in my hands." I said, "Of course you don't...but just what is it that you don't have?" The hands came forward, one holding an apple. The little boy then recognized his strategic misstep and laughed. The apple was for me, and we feasted on it, and on the joys of

make-believe. The arbitrariness thesis was de Saussure's claim he didn't have something he concealed so well that he didn't know he did—or had it. He suffered from severe iconic deficiency.

Same with 'substance'. If only Saussure hadn't mentioned it! He couldn't get rid of it, so he supposed no one else could, and he denied there was any such thing. 'Thing'? There we go again... enjoying make-believe.

Deconstruction punctures make-believe.

• • •

Structuralism is still very much alive—in that make-believe called generative grammar. Syntax is 'form'. No deconstruction there so far. And all for the sake of that ultimate-undeconstructible—'Universal Grammar'. With those caps, of course; that is part of the immunity strategy against deconstruction. 'Orthographic inoculation', I call it.

What does deconstruction of structuralism look like, in linguistics? I ponder the following. Language is mere form. All right. Distinctive features all over the place, in any language L: phonologically, morphologically, syntactically, semantically, pragmatically. But of all those things only diacritical **pragmatics** is new. In the celebrated phrasing of Du Bois: 'Speakers do best what they do most'.

If speakers of L dislike (by and large) New Information (NI) agents, L is ergative. If they look at NI non-agents as within the scope of what agents do to them, L is accusative. If they want neither, L is active-stative (generativists contumaciously continue to call this 'ergative'). I also ponder that all this has little if anything to do with 'theme-continuity': Humans (and thus prototypically agents) are the most important 'theme' in any L—after all, that is what animacy hierarchy is all about. This 'theme continuity' translates as 'topic continuity' (in Givón's sense) in discourse analysis; that is to say, agents are the most continuous topic in any L.

But the organization of NI and Old Information (OI) is based on what speakers do most, and what they do most is based on what they assume hearers are aware, and this assumption is culturally a self-fulfilling prophecy. In other words, the entire picture shows the network of relations among speakers and hearers of L. Speakers and

hearers are not self-contained 'substances' (let alone 'minds'), They are points in a network. You and I are sources, sums, and stabilizers of relationships. Language typology is, I like to think, mainly about that.

In other words, the deconstruction of structuralism is functionalism. Perhaps no linguist was so remote from functionalism as de Saussure, and yet his approach inevitably leads to functionalism.

* * *

The diacritical view is seductive. It is because it is convincing. It is therapeutic. Bodies with 'substance' collide; points never do, for their 'value' is in how they relate to others, and others to them. This is true of human identity. Buddhism has taught for millennia that we are all such points—nonentities, bathing in those relations—and thus, 'values' rather than 'substances'. Zen has tried to bring that ideal to perfection. No wonder Zen is mystical.

But structuralism never was. Western structuralism is Zen without any of its mysticism, for structuralism is interested only in relations of an L which doesn't relate to speakers or hearers, or even to anything people speak about. Structuralists worry about relations within an L unrelated to anything else. It won't help then to 'relate' L to something even more unrelated to anything at all: that 'mind'—that is perhaps the only approach that makes things even worse.

Structuralism, I muse, becomes functionalism when inspired by the art of not overlooking the obvious. ■

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Reflections on Isthmus Zapotec Inflection

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Introduction

1. Elson and Pickett (1989:212-213 or 1967:183) give the following verbal prefixes for Isthmus Zapotec:

ASPECT	SET 1	SET 2	SET 3
habitual	ru-	ri-	ri-
completive	bi-	bi-	gu-
future	zu-	za-	za-
progressive	ku-	ka-	ka-
potential	gu-	gi-	Ø-
unreal	nu-	ni-	ni-
perfect	wa-	wa-	wa-

Table 1: Isthmus Zapotec verbal inflection

This paradigm involves seven morphemes, having a total of 14 different allomorphs. Using the subcategorization feature *set* with three possible values (1, 2, or 3), these are as follows:

/rV-/	'habitual'	[ru-] for [1 set] [ri-] for [{2,3} set]
/bV-/	'completive'	[bi-] for [{1,2} set] [gu-] for [3 set]
/zV-/	'future'	[zu-] for [1 set] [za-] for [{2,3} set]
/kV-/	'progressive'	[ku-] for [1 set] [ka-] for [{2,3} set]
/gV-/	'potential'	[gu-] for [1 set] [gi-] for [2 set] [Ø] for [3 set]
/nV-/	'unreal'	[nu-] for [1 set] [ni-] for [{2,3} set]
/wa-/	'perfect'	([wa-] for all sets)

Many regularities—some only partial—are not captured by simply listing the allomorphs. Leaving aside certain exceptions (to which we will return directly), note the following:

1. Set 1 forms have the vowel /u/.

2. For any given tense/aspect/mood, the vowel is the same for the set 2 and set 3, and it is usually /i/.

3. Each tense/aspect/mood is indicated by a particular consonant, /r/ for habitual, /b/ for completive, /z/ for future, /k/ for progressive, /g/ for potential, /n/ for unreal, /w/ for perfect.

The following are exceptions to these regularities:

1. The perfect is /wa-/ in all three sets.

2. The completive, set 1 form has /i/ (rather than /u/).

3. The completive, set 3 form is /gu-/ (where we expect the consonant to be /b/ and the vowel to be /i/).

4. The potential, set 3 form is Ø (where we expect the consonant to be /g/ and the vowel to be /i/).

5. The future and progressive, set 2 and 3 forms have /a/ (rather than /i/).

These observations—both the partial regularities and the exceptions—may be apparent from a study of the paradigm, but they are not incorporated in any way within the solution itself. We need an approach which allows capturing the generalizations as well as stating what is exceptional.

In this note I intend to show how this can be done in the Extended Word and Paradigm (EWP) theory of morphology. (This theory was first outlined in Anderson (1977). Thomas-Flinders (1981) includes several applications.)

Marlett and Pickett (1987) give a detailed analysis of Isthmus Zapotec inflections, one which considers a wider range of data than considered here. It derives the aspectual forms from abstract, underlying forms using various phonological rules. This note is not intended to challenge Marlett and Pickett's analysis but simply to

illustrate—within the restricted data of Table 1—the usefulness of EWP morphology for capturing generalizations.

An Alternative Approach: EWP

2.1 How nodes get features. Zapotec tense/aspect/mood (TAM) categories are expressed as verbal inflections. To implement this within the EWP theory, the categories are added as features to the verbal nodes of a syntactic tree a rule of the following sort.

$$V \longrightarrow [\left\{\begin{array}{l} habitual \\ completive \\ future \\ progressive \\ potential \\ unreal \\ perfect \end{array}\right\} tam]$$

Within the feature that is added, 'tam' is the name of the feature and 'habitual,' 'completive,' etc. are possible values for it.

The lexical entries of verbs must carry a feature indicating the verb's class. When a verb is inserted into a syntactic tree, this feature is inserted with it.

In light of the three columns of Table I, one might assume that this class-marking feature has three possible values: [1,2,3 set]. However, sets 2 and 3 share properties not shared with set 1. To capture the many forms that are common to class 2 and 3, we choose to use two rules: first, V→[±classI], and second, [–classI]→[±classII]. These are expressed graphically in Figure 1:

$$V \longrightarrow \left\{\begin{array}{l} +classI \\ -classI \longrightarrow \left\{\begin{array}{l} +classII \\ -classII \end{array}\right. \end{array}\right.$$

Figure 1: Feature system

This system of features allows us to refer to set 1 as [+classI], to sets 2 and 3 jointly as [–classI], to set 2 as [+classII], and to set 3 as [–classII]. (Note that [±classII] implies [classI].)

In a syntactic tree, the preterminal node of a class I verb which is specified as habitual would look like the following:

$$V\left[\begin{array}{ll} habitual & tam \\ +classI & \end{array}\right]$$

Now inflectional 'spell out' rules must be fomulated to guarantee that /ru-/ is prefixed to the root/stem dominated by such a node.

2.2 Realizing inflectional features. Inflectional spell-out rules are formulated as ordered, disjunctive sets; see Anderson (1986). For Zapotec, we need three sets: 1a-c, 2a-d and 3a-f. Rules 1a-c add a complete prefixes for exceptional forms (clearing the way for subsequent generalizations). Rules 2a-d specify add a vowel and rules 3a-f add a consonant.

In addition to each block of rules being ordered and applying disjunctively, the rules of set 1 are disjunctive with respect to sets 2 and 3. That is, if a rule of set 1 applies then no other rules apply.

On the other hand, if no rule of set 1 applies, then one rule from set 2 and one rule from set 3 must apply. Because each of these blocks of rules applies disjunctively, each form receives only one vowel and one consonant.

The first block of rules are as follows:

1a. $\left[\begin{array}{ll} perfect & tam \end{array}\right]$
$/X/ \longrightarrow /wa + X/$

1b. $\left[\begin{array}{ll} completive & tam \\ -classII & \end{array}\right]$
$/X/ \longrightarrow /gu + X/$

1c. $\begin{bmatrix} potential & tam \\ -classII & \end{bmatrix}$
$/X/ \longrightarrow /X/$

1a captures the commonality across classes in the perfect form, thereby removing them as obstacles to other generalizations. 1b deals with the completive class III form (/gu-/), and 1c deals with the null allomorph for potential class III.[1]

The second block of rules is as follows:

2a. $\begin{bmatrix} completive & tam \\ +classI & \end{bmatrix}$
$/X/ \longrightarrow /i + X/$

2b. $\begin{bmatrix} \{future, progressive\} & tam \\ -classI & \end{bmatrix}$
$/X/ \longrightarrow /a + X/$

2c. $[\ +classI\ ]$
$/X/ \longrightarrow /u + X/$

2d. $[\ -classI\ ]$
$/X/ \longrightarrow /i + X/$

2a deals with the exceptional completive class I form, and 2b deals with the exceptional future and progressive class II and III forms. With these out of the way, 2c expresses the generalization that the vowel of class I forms is /u/, and 2d expresses the generalization that the vowel of class II and II forms is /i/.

The third block of rules is as follows:

3a. [*habitual tam*]
$/X/ \rightarrow /r + X/$

3b [*completive tam*]
$/X/ \rightarrow b + X/$

3c. [*future tam*]
$/X/ \rightarrow /z + X/$

3d. [*progressive tam*]
$/X/ \rightarrow /k + X/$

3e. [*potential tam*]
$/X/ \rightarrow /g + X/$

3f. [*unreal tam*]
$/X/ \rightarrow /n + X/$

Rules 3a-f capture the regularity with which consonants correlate with different tense/aspect/moods.[2]

2.3 How the rules apply. Let us return to the example node given above, assuming that it dominates a root /ka/:

$$\begin{array}{c} V \\ \left[\begin{array}{ll} habitual & tam \\ +class1 & \end{array}\right] \\ | \\ /ka/ \end{array}$$

No rule from set 1a-c applies, since this node's features do not match those of any of these rules. Considering rules of set 2a-d, the first that applies is 2c, which prepends /u/; the result is:

$$\begin{array}{c} V \\ \left[\begin{array}{ll} habitual & tam \\ +class1 & \end{array}\right] \\ | \\ /uka/ \end{array}$$

Since the rules are disjunctively ordered, right after 2c applies, we proceed to block 3. Taking each rule in turn, the first that applies is 3a, which prepends /r/:

$$\begin{array}{c} V \\ \left[\begin{array}{ll} habitual & tam \\ +class1 & \end{array}\right] \\ | \\ /ruka/ \end{array}$$

After this rule applies, we immediately exit this block of rules. Since there are no more rules, the inflected word is /ruka/.

2.4 An improvement. Elson and Picket (1989:213) state two phonological rules applying after these tense/aspect/mood prefixes are added to roots.

One states that /za-/ 'future' becomes /zi-/ before a vowel-initial root. However, in light of the generalization that /i/ is characteristic of classes II and III, it is simpler to take /zi-/ as the basic form and rephrasing the rule as, "/zi-/ 'future' becomes /za-/ before consonant-initial roots."

Alternatively, the following rule—incorporating the sensitivity to the initial consonant—could be added to set 1:

1d. $\begin{bmatrix} future & tam \\ -class1 & \end{bmatrix}$

$/CX/ \longrightarrow /za + CX/$

On either of these alternatives rule 2b no longer needs to account for the future, so it can be simplified to the following:

2b'. $\begin{bmatrix} progressive & tam \\ -class1 & \end{bmatrix}$

$/X/ \longrightarrow /a + X/$

The other phonological rule is as follows: insert /y/ between /ka-/ 'progressive' or /wa-/ 'perfect' and a following vowel-initial root. However, if /zi-/ rather than /za-/ is chosen as the basic form of the future, then /ka-/ 'progressive' and /wa-/ 'perfect' are the only forms ending in /a/, and it is unnecessary to mention the specific morphemes. The rule can be stated simply as, "insert /y/ between /a/ and a vowel-initial root."[3].

A Concluding Comparison

3. Consider the relative complexity of listing allomorphs on the one hand and inflectional spell out rules on the other.

The former (traditional) approach takes 14 allomorphs and two morphophonemic rules (or perhaps 13 allomorphs if some device is used to avoid the null allomorph).

The Extended Word and Paradigm approach takes 13 'spell out' rules, one morphophonemic rule and one phonological rule (or 14 'spell out' rules and one phonological rule). Additionally, it involves rule ordering and disjunctive rule application. However, virtually none of the rule ordering needs to be stipulated since this is predicted by the principle that more specific rules precede more general ones (Anderson 1986).

It seems, therefore, that the two solutions differ very little in complexity. However, the EWP approach is a better solution because the rules express directly the regularities in the paradigm, as well as the exceptions to those regularities.

The moral: we should not be content to simply list allomorphs.

NOTES

[1] Anderson (1977) claims that rules which makes no phonological change, such as 1c, are necessary, even though somewhat inelegant.

[2] A rule for /w/ 'perfect' is not necessary because of rule 1a.

[3] Even this might be unnecessary in an approach such as that of Ito (1989).

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Remarks and Replies

Remarks on
Head Shift: A Diary Entry
by John Verhaar (*NL 62*)

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In response to John Verhaar's article in *NL 62*, let me repeat an example that one of my classmates presented in a class in the 1965 Linguistic Institute at Ann Arbor, Michigan:

> Consider the phrase 'a bottle of beer' in the two following sentences:
>
> She poured a bottle of beer over his head.
>
> She broke a bottle of beer over his head.

In the first example, the head of the phrase is **beer** because of the verb **poured** but is **bottle** in the second example because of the verb **broke**. I think what we have here is a mismatch between syntax and semantics. The syntactic head of the phrase is **bottle** and the prepositional phrase of **beer** is a qualifying postmodifier in an ordinary English noun phrase. Semantically, either noun can be **head**, as dictated by the context. Is such a non-one-to-one relationship between syntax and semantics a linguistic heresy?

6th International Conference on Functional Grammar
University College of Ripon and York St. John, York, England
22-26 August 1994
Theme: **Pragmatics and Discourse in Functional Grammar**. There will be at least one poster session for displaying work. Deadline for submitting abstracts (Jan. 15, 1994) was past before publication of this issue of *NL*.
Address queries to Dr. C. S. Butler, Dept. of English Language and Linguistics, University College of Ripon and York St., John, Lord Mayor's Walk, York, YO3 7EX, England (tel. 0904-616778, fax 0904-612512).

4th International Pragmatics Conference

Kobe, Japan—July 25-30, 1993

Regina Blass
SIL-Burkina Faso/Niger Branch

The International Pragmatics Conference, which the International Pragmatics Organization (IPrA) organizes every third year in different countries of the world, took place this time at Shoin Women's University, Kobe, Japan. Six hundred participants were there from about thirty countries. Since the conference was held for the first time outside of Europe, about half of the participants were Asians. This very high number made the conference a unique and enriching experience to Westerners.

The conference was organized by an international committee of linguists including John Gumperz, Ferenc Kiefer, Sandra Thompson, Jef Verschueren, etc., and a local Japanese committee which included Masayoshi Shibatani.

The main theme was 'Pragmatics and Culture'. Minor subjects were 'Second language acquisition' and 'Translation', among others. The conference activities were organized as plenary lectures followed by panel sessions and lectures with five or six sessions going on at the same time. There was also a poster room where a number of linguists with papers on subjects other than pragmatics and culture could display their papers during one entire day and discuss them between activities. There were also book displays, mainly of new publications. There were a number of SIL books displayed, although I was unable to identify any other SIL personnel in the conference.

Main speakers for the plenary lectures included Charles Fillmore, Masayoshi Shibatani, Robin Lakoff, Jacob Mey and Anna Wierzbicka.

In order to make it possible for everyone to gain some cultural insights into the host culture and to give the conference a distinctly Japanese flair, lessons in calligraphy, flower arrangements, and the

Japanese flair, lessons in calligraphy, flower arrangements, and the tea ceremony were arranged during lunch hours. The traditional linguistic banquet was an array of Japanese exotic beauty and, at the end, every participant was given a beautiful book about Kobe. The Japanese were superb hosts.

Fillmore introduced the pragmatics of constructions, showing how constructions with conceptual meaning can develop into constructions with conventional meaning, having a mainly pragmatic function. As one example in the expressions, 'let alone' and 'what's it doing (on the table, for instance)', the individual words do not carry the meaning, rather the whole construction leads to a pragmatic interpretation. He showed that there might be an in-betweenness of conceptual and conventional meaning, therefore pointing to an unclear line between Semantics and Pragmatics. Moreover, he noted that, because of their conceptual versus conventional meaning, constructions can give rise to misinterpretation when translated into other languages.

Shibatani, attacking Chomsky, argued that Syntax cannot be treated autonomously but is always dependent on meaning.

Lakoff spoke about 'The Subversive Pragmatist', quoting Lewis Carroll in various writings, showing her distrust in the Grician and other logically based theories by claiming that no theory of pragmatics and culture can be constructed. She described Carroll's opinions as follows:

> In that way he functions as a potent critic of human culture and its rationalizations, showing that the way we perceive, create, and interact with the interactive and cognitive universe is neither inevitable, nor logical, nor necessarily right.

When asked how she would define 'Pragmatics', she said: 'Pragmatics is interesting stuff about language'(!).

Wierzbicka introduced 'Cultural Scripts' and gave many examples of how they could differ from culture to culture. Thus an American child would have the script, 'It is good to show that one has a different opinion from someone else', and a Japanese child would have the script, 'It is not good to show that one has a different opinion from someone else'.

Mey, similar to Lakoff, approached the subject from a philosophical point of view. In his view pragmatics ought to be dealt with more proactively than hitherto done—thinking about future contexts rather than merely contexts of the past or present. As a good example he quoted Posner's article on *Warnungen an die ferne Zukunft: Atommüll als Kommunikationsproblem* (Warnings to the future far ahead: Atomic waste as communication problem).

Most people who had come to learn how culture and pragmatics are linked were disappointed. These lectures contributed little to the subject. The most informative was Fillmore's Pragmatics of Construction, although it was the most humble in claims. It gave some empirical insight into how culture influences structure and meaning. Wierzbicka's presentation might have been more relevant if she had shown how her 'Cultural Scripts' work mentally, how they interact with what is said and how they affect form and interpretation, rather than merely giving a series of examples of what we already know: i.e. that different cultures have different values. Mey's and Lakoff's presentations, though creatively constructed and dealing with unusual subjects, contributed little to an understanding of how culture and pragmatics go together, or even about pragmatics at all. In fact, one might suspect they had really nothing new to say about the subject and therefore diverted to *Alice in Wonderland* and 'atomic waste'.

There were panel sessions on the subjects of 'Computer mediated communication', 'Mental spaces', 'Politeness', 'Referent and context accessibility', 'Relationship between conceptual and empirical issues', 'Methods for eliciting speech in pragmatic research', 'Contrastive pragmatics and Japanese language teaching'.

The panels usually had more attendants than the simple lectures, the latter being about the subject of politeness or second language acquisition, interesting to a limited group anyway. Moreover, in the panel sessions, additional speakers had been asked to prepare comments to the talks beforehand. Sometimes these speakers were experts on the subject as well as coming from a different theoretical background. This way the hearer got to know the pros and cons of what had been said.

What was interesting was that in the panel sessions the ones having least to do with culture, such as those on Referent and Context Accessibility, had the highest attendance. It might have been because they had such well known people as Jens Allwood, Ellen Prince, and Jeannette Gundel, but it also might have been that most of the simple lectures approaching the theme of culture and pragmatics did not promise the answers that people expected, so pragmatists preferred to attend the more theoretically oriented lectures which promised useful information.

Since almost all papers not dealing with culture had been assigned to be 'posters', there were a number of papers of very high quality among them covering almost every aspect of pragmatics. In fact, I found that some of the poster presentations alone made the journey worthwhile for me. I also found the poster method very helpful since it was possible to read the articles beforehand and then meet the authors in person to discuss the subject matter. It was also easier to obtain copies of the articles. I was able to find some papers with interesting ideas on discourse and exciting data in the area of particles from a number of languages from different parts of the world.

Although 'translation' was one of the subjects of the conference, there were few papers on the subject and their abstracts were uninviting; nothing nearly as sophisticated as Gutt's *Translation and Relevance*.

It seemed there were two groups of people in the conference: the sociolinguists or ethnologists and the 'real' pragmatists, in the sense of being interested in a theory of utterance interpretation. The former group, dominating many of the lectures, dealt with culture in such a way that they pointed out communication problems and how cultures and therefore utterances differ. They did not propose any coherent theory. (The latter group almost avoided the subject of culture and dealt with theoretical issues such as 'Referent and context accessibility'. Some raised the issue of how pragmatics could be more formal without being unnaturally formal, as in some of the formal semantic and pragmatic theories such as Montague Semantics and Situational Semantics.) So the question of how pragmatics and culture are linked stayed largely unaddressed.

Those interested in pragmatics as a theory felt that there was a great deal going on in the conference which would have been more suitably placed in a conference on Ethnology or Sociolinguistics. However, the IPrA leaders defended themselves by saying that they like to keep the subject broad in order to cover what they call 'The pragmatic perspective'.

In my opinion, conferences like this are there in order to make advances in its academic field. This means that one also has to filter out what one has recognized as not relevant. Since the preceding conferences in Antwerp and Barcelona (which I also attended), had already shown the direction a theory of pragmatics has to go if it should have an explanatory and scientific basis, many of the papers simply should not have been accepted. As it was, many participants had the impression that the same questions were being asked and the same problems presented as in previous years without taking any notice of the answers already found.

Maybe it is because of the 'pragmatic perspective view' of the organizers that the big names in Pragmatics, such as Levinson, Horn, Leech, Ducrot, Sperber, Wilson, Kempson, and van Dyke were absent, although their names were quoted many times and their theories mentioned as adequately addressing the problems.

Relevance Theory was mentioned often and proposed as an answer to most of the referent and context accessibility problems. When it became known that I had studied under Deirdre Wilson, I was able to talk to a number of people interested in the theory. Quite a few of them were Japanese linguists. While talking about Relevance Theory I was often asked to which university I belonged, and was able to introduce SIL to many linguists. ■

Congratulations to the following SIL members who completed Ph.D's in Linguistics during 1993:
Andy Black (Mexico Branch, formerly Peru Branch) – University of California at Santa Cruz
Rick Floyd (Peru Branch) – University of California at San Diego
Larry Hagberg (Mexico Branch) – University of Arizona

24th Annual Conference on African Linguistics

Mike Cahill
SIL—West Africa Group/Ghana

The 24th Annual Conference on African Linguistics (ACAL) was held July 23-25, 1993 on the campus of Ohio State University. Around 130 participants attended.

On the agenda were 118 papers, and only a few failed to materialize. Quite a few grad students presented papers. The quality of these was in general fairly good, with many of them dealing with or at least referring to recent theoretical issues, such as the recent 'optimality theory' in phonology.

Larry Hyman was the invited plenary speaker giving a lively address titled 'The Luganda syllable revisited'. Besides the Luganda data, he also shared perspectives about the advantages of working on a language long-term. He has worked on Luganda for ten years, and advocated more long-term commitment to a language, rather than 'getting just the fun and obvious things' out and then moving on to something else. I found this a refreshing attitude from a respected phonologist. Also refreshing was his admission that the facts of Luganda had forced him to change his approach to phonological rules, that cyclic, lexical vs. postlexical applications of a single rule, etc., really are needed to account for some data.

Three or four parallel sessions were running simultaneously, giving some hard choices to make on which paper to listen to. The ACAL this year could almost have been called a conference on Bantu phonology. Of the 118 papers scheduled, 53 were primarily phonological. The majority of these were from Bantu languages, with a smattering of Kwa, Chadic, and Gur. Thirty-four papers were on syntax, and the remaining 31 were divided among historical, language and culture, lexicon, semantics, and sociolinguistics.

SIL had four participants, all giving papers: Rod Casali ('Labial opacity and roundness harmony in Nawuri'), Keith Snider ('Tonal downstep or upstep?'), and Mike Cahill ('Diphthongization and underspecification in Konni'). These were all fairly well received. Another plus for SIL is the good reputation that the CECIL hardware and software has. Several non-SIL people are using it, and the tone extraction portion of CECIL, especially, is recognized as good as anything available commercially (and much cheaper, of course!).

David Odden had the ACAL very well organized with grad students at Ohio State doing a tremendous amount of helping on everything from registration to the book table to the African banquet on Saturday night. The chairpersons for each session kept the papers moving and each session on time; thus it was possible to move from one session to another between papers.

4th Southeast Asian Linguistics Society Conference—1994
Theme: Southeast Asian Linguistics
Ramkhamhaeng University, Bangkok
May 23-25
Payap University, Chiangmai
May 26-27
Abstracts should be received by February 15, 1994 and contain heading with full name and complete address, title of the papers and a one- page summary of topic, approach, and major conclusions. Manuscripts should be completed by March 20, 1994 and be typed and camera ready. Send 3 copies to: Dr. Udom Warotamasikkadit; Dept. of English & Linguistics; Faculty of Humanities; Ramkhamhaeng University; Bangkok, 10240 Thailand.
Ph. (662)318-0054; Fax (662)318-0904.

21st International Systemic Functional Congress
Theme: Functions of Language
University of Gent, Belgium—August 1-5, 1994
Deadline for submitting proposals (Dec. 15, 1993) was past before scheduled publication of this issue of *NL*. Send correspondence to: A.-M. Simon-Vandenbergen; Dept. of English, University of Gent; Rozier 44; 9000 Gent, Belgium. Tel: +32 9 264.37.87; Fax: +32 9 264.41.84; E-Mail: vdbergen@engllang.rug.ac.be

3rd International Cognitive Linguistics Association Conference

Eugene H. Casad, SIL—Mexico Branch
and
Rick Floyd, SIL—Peru Branch

The International Cognitive Linguistics Association (ICLA), which meets every two years, held its third meeting 18-24 July 1993, in Leuven, Belgium with some 200 in attendance. The conference saw a total of more than 100 papers presented in five parallel sessions, and also featured 10 plenary lectures given by internationally recognized scholars representing a fully interdisciplinary group of linguists and psychologists. The Plenary speakers included Dirk Geeaerts, George Lakoff, Erica García, Bernd Heine, Len Talmy, Joan Bybee, Douglas Medin, Dedre Gentner, Ron Langacker and René Dirven.

Of the ten plenary sessions, we will only mention three that illustrate the breadth of notions encompassed by the conference. Ronald Langacker's discussion on 'Transparency' showed how the analysis of certain 'raising' phenomena contrasted in generative and cognitive approaches. He argues that the relevant syntactic structures are a straightforward consequence of their semantic structure and can be analyzed in the cognitive framework without recourse to deep structures. Joan Bybee discussed 'Mechanisms of semantic change in grammaticization' dealing specifically with the various stages in a grammaticalization chain at which metaphorical extension, 'pragmatic strengthening' and 'absorption from context' apply. George Lakoff's talk ('Cognitive cultural theory') had as its basic idea that the fundamental notions in cognitive semantics extend beyond the traditional bounds of syntactic and semantic analyses to the analysis of such diverse institutional concepts such as marriage, democracy and even game theory in international relations.

Three SIL members presented papers: Eugene Casad ('Where did *Nu* Go in Cora?'), Rick Floyd ('Evidentials in questions?') and David Tuggy ('When metaphors collide').

The richness of the field as it continues to develop is graphically demonstrated by the content of the section lectures, each 30 minutes long, with ten minutes for questions. These were organized into 22 topics as follows:

1. Patterns of construal
2. Transitivity and clause structure
3. Discourse studies
4. Lexical-semantic relations
5. Universal and typological tendencies
6. Possessives and case roles
7. Iconicity and markedness
8. Causative and purposive constructions
9. Morphology
10. Knowledge representation
11. Comparing with alternative theories
12. Grammaticalization and historical changes [Eugene Casad's paper was in this section]
13. Conditionals and subordination
14. Idioms and constructions
15. Lexical studies
16. Evidentials and modality [Rick Floyd's paper was in this section]
17. Metaphor [David Tuggy's paper was in this section]
18. Psychological models of categorization
19. Prepositions
20. Acquisition and learnability
21. Reflexives and pronouns
22. Anthropological linguistics

On the whole, the papers given at this conference were of a high academic caliber. As is customary, a selection of the papers will be submitted to the refereeing process and those that pass will appear in a volume that will come out in the series, **Cognitive Linguistics Research**. The editor for this volume will be Leon G. de Stadler of the University of Stellenbosch, Republic of South Africa.

The next meeting of the ICLA is scheduled to coincide with the Summer Linguistics Institute in Albuquerque, NM during the third week of July 1995. For information concerning the ICLA or *Cognitive Linguistics*, contact:

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Sherman Willcox, University of New Mexico, will be coordinating that meeting. Eugene Casad will be involved in the handling of the abstracts of the papers that will be submitted for possible presentation there.

Although most participants were not aware of it, there was a satellite meeting at the Department of Psychology of the University of Leuven which involved a number of people working in the field of Cognitive Science. Eugene Casad was privileged to attend the Saturday session called 'Psychological Models of Categorization' and meet a few of these people. The papers were interesting and showed clearly that the psychological underpinnings of the approaches of Ron Langacker and George Lakoff are very solid indeed. As one participant herself put it, this conference was the start of a real appreciation of the work of Langacker and Lakoff and of its acceptance within the broader field of Cognitive Science. ■

American Association for Applied Linguistics
Baltimore, Maryland
March 5-8, 1994
Deadline for submitting abstracts (September 17, 1993) was past before scheduled publication of this issue of *NL*.

Plenary Speakers
Ronald Carter, Patsy Lightbown, Catherine Snow, Janet Swaffar, Bernard Spolsky

Featured Colloquia
Some recent advances in second language acquisition
Transfer from L1 to L2 literacy
Assessment of immigrants' and minorities' language competency
Co-Construction
Discourse at work
Stylistics and the teaching of literature.

Reviews

A-Morphous morphology. By Stephen R. Anderson. Cambridge: Cambridge University Press, 1992. 384 pp. Hardcover $79.95, Paper $24.95.

Reviewed by Joan Baart
SIL—West Eurasia Group

In 1976, when I started my studies at Leiden University, The Netherlands, our class was introduced to general linguistics by Professor E. M. Uhlenbeck, a renowned Dutch scholar of the pre-Chomskyan generation. Some of the ideas Uhlenbeck taught us can be summarized as follows: A language is a system of simple and complex units of form and meaning (**signs**). In this system, two units have special status: the **word** and the **sentence**. The sentence is special because it is the minimal unit of communication. The word is special because it is the main building block for constructing sentences.

Now, every language has certain means to increase its stock of words. These may involve (disregarding non-morphological means such as borrowing) combining existing words into new words (**composition**), or deriving a new word on the basis of an existing word through a formal modification (such as adjoining an affix). We can study these word formation processes by compiling lists of word pairs in which a constant formal difference correlates with a constant semantic (and/or syntactic) difference. We could, for example, look at **fair-unfair, certain-uncertain, armed-unarmed, common-uncommon**, etc., and observe that there is a constant formal difference (absence vs. presence of the prefix **un-**) which correlates with a constant difference in meaning (the one form refers to a quality, the other to the negation of that quality). If we think there is enough evidence to assume that this correlation is systematic, we may write a rule which describes how the one word is formed on the basis of the other, and how the meaning of the resultant word is derived from the meaning of the first word. Such a rule characterizes a **morphological process.**

What I have sketched in the preceding paragraph is an approach to morphology which takes the word as its basic unit and studies how words are related through morphological processes. This contrasts with an approach that takes the morpheme as basic and studies how morphemes combine into words. (At this point, let me add the disclaimer that I have summarized bits of what I remember from Uhlenbeck's lectures in my own, rather than his words.)

Several years later, when I participated in a project to build a computer system for morphological analysis of Dutch, Uhlenbeck's teachings were no longer in the forefront of my mind. Our parser consisted of a morpheme dictionary, and of procedures for segmenting a complex word into its constituent morphemes. Little did we question the validity of the model underlying this approach. Sometimes we ran into problems related to the fact that not all morphological phenomena can be reduced to simple concatenation of morphemes (modulo allomorphic changes), and that the definition of the morpheme itself seemed elusive.

Again later I found myself in SIL classes being introduced to linguistic field methods. We learned how to segment words into morphemes, how to put morphemes into morpheme charts, and how to account for simple allomorphic variation. It seems that the morpheme pervades our grammatical thinking. It is everywhere. Even Shoebox, an extremely useful computer program for field linguistics, wants me to tell it about morphemes, and assumes that all the words in my language can be neatly chopped up into a string of such things.

Nowadays I am involved in analyzing a language where some major, highly productive morphological processes involve stem modification rather than addition of affixal material. In Kalami, *look* means 'small (masc.)' and *leek* means 'small (fem.)', *khaant* means 'he is eating' and *kheent* means 'she is eating', *kukur* means 'rooster' and *kikir* means 'hen'. Should I postulate a 'replacive morpheme' with variants **-ee-** and **-i-** which replace the stem vowel(s) of the masculine form in order to derive the feminine form? Doesn't that somehow feel like trying to force upon the data a model that was really meant for something else?

When I read the first chapters of the book under review, I had a strong feeling of *déjà vu*. After some time I realized that it was not just a feeling. I had seen quite a few of the basic ideas before, namely in my early linguistics classes in university.

With this lengthy introduction I am certainly not dismissing Anderson's work as intellectual piracy. Anderson accurately documents his sources, and acknowledges several other (generative) morphological theories that share important features with his own model. The point is rather to draw attention to the legitimacy of this kind of approach, which goes against the basic linguistic assumptions of some.

The book presents a theory of morphology which the author calls *A-Morphous Morphology*. The theory 'emphasizes the notion of morphology as the study of relations between words, rather than as the study of discrete minimal signs that can be combined to form complex words'. The theory consequently 'dispenses with morphemes' (p. 1). In addition:

> ...the view of word structure as a system of rule-governed relations between words leads to the elimination of much of the apparatus of word-internal boundary elements and constituent structure common in morphological discussions. [As such it is] a theory that minimizes the amount of (non-phonological) form that is assigned to words (p. 2).

In 14 chapters different aspects of the theory are discussed. Chapters 1-3 provide motivation for Anderson's basic assumptions; chapters 4-11 flesh out the theory in the areas of inflection, derivation and composition; the final three chapters (12-14) discuss language typology, diachronic morphology, and computational morphology, and the interaction of these fields with the present theory.

Chapter 1 (The study of word structure) gives a brief sketch of the development of ideas concerning words and their structure from Ferdinand de Saussure via American structuralism to early generative grammar. This chapter also deals briefly with the question of how 'word' is to be defined; it does not really answer that question, but expounds the idea that a grammatical, rather than a phonological, definition of 'word' is relevant for morphology.

In early generative grammar, the morpheme was viewed, on the one hand, as a fundamental linguistic unit, while on the other hand, morphology itself dissolved into other components of grammar: A syntactic component handled the proper combination of morphemes into larger units, and derived the right surface form of a morpheme in a given context.

In the following two chapters Anderson attacks these positions. Chapter 2 (Why have morphology at all?) presents arguments for the assumption of morphology as a distinct part of grammar. Among other things the chapter argues against some recent analyses of incorporation phenomena in which syntactic rules are allowed to manipulate word-internal structure. In Chapter 3 (Is morphology really about morphemes?) the notion of the morpheme is actually dismissed.

There are two lines of argument against the morpheme. One involves fundamental problems with the definition of the morpheme as a minimal unit of form and meaning. These problems concern the existence of **infixes** which break up stems into discontinuous parts; **circumfixes** which themselves are discontinuous elements, one bit of which is put in front of a stem and the other bit after the stem; **empty morphs**, which are morpheme-like elements that appear to lack any content; **cumulative morphs** where several 'meanings' are expressed by one form; and other cases where it is impossible to neatly line up subparts of form with subparts of meaning.

The other line of argument against the morpheme is that the languages of the world sometimes exhibit morphological operations that really have nothing to do with concatenation of forms at all. An example is **subtractive morphology**, as in Papago where:

> ...in most cases the perfective can be derived from the imperfective by dropping the final consonant (**him** 'walking' - **hi:** 'walked', **hi:nk** 'barking' - **hi:n** 'barked', etc. (p. 65).

Another example is **metathesis**, as in Saanich, where the 'actual' form of a verb is derived from the 'non-actual' form by revising the order of a vowel and a consonant (p. 67).

Fine so far, but is there a viable alternative to morpheme-based theories, and can it do for us what our present theories do? This is what the rest of the book is about. The chapters sketch the contours of a word-based, generative morphological theory and illustrate its application.

Chapter 4 (The interaction of morphology and syntax) defines the notion of **inflection** as 'the domain in which the systems of syntactic and morphological rules interact' (p. 74). It introduces the construct of a 'Morphosyntactic Representation' as an 'interface' between morphology and syntax. In Chapter 5 (The theory of inflection) the material in the previous chapter is made more specific. Anderson discusses how different types of agreement and government can be handled, and also how an ordered set of morphological rules can derive the phonological form of an inflected word. Chapter 6 (Some complex inflectional systems) illustrates the theory with analyses of two languages: Georgian and Potawatomi.

Chapter 7 (Morphology in the lexicon: Derivation) discusses the nature of derivational rules. This chapter is largely a review of existing literature and theory.

Chapter 8 (Clitics are phrasal affixes) deals with the status of **clitics**, and relegates a subset of these elements to an area of grammar distinct from both morphology and syntax, which might be called the 'morphology of phrases'.

Chapter 9 (The relation of morphology to phonology) shows how phonology can dispense with word-internal boundary elements, if we adopt certain premises central to the theory of Lexical Phonology, namely (a) that 'morphological and phonological rules interact in a cyclic fashion, with phonological adjustment following each morphological operation'; and (b) that 'rules of each sort are divided into subsets, such that a given morphological operation leads to the potential application only of the appropriate subset of the phonology' (p. 225).

Chapter 10 (How much structure do words have?) continues to argue for the elimination of much of the internal grammatical structure which words are assumed to have. It considers the extent to which rules of semantic interpretation, syntax, morphology, and phonology

might need to refer to the internal structure of words; then it goes a step further and questions if such internal structure exists at all. For most cases, this question is answered in the negative, but Chapter 11 (Composites: Words with internal structure) argues that in the case of composition we do have reasons to assume such structure. Consequently, a new type of rule is introduced: 'Word Structure Rules'. This is in addition to the 'Word Formation Rules' that were needed for inflection and derivation.

Chapter 12 (Morphology and the typology of languages) for the larger part reviews Sapir's typology of word structure in the light of the present theory. The chapter concludes that the difference between doing typology and doing theory is illusory.

Chapter 13 (Morphological change) presents an insightful discussion of how morphological rules may come into being and how they may change. The development of ergative morphology in several languages is discussed at length.

In Chapter 14 (Morphology as a computational problem) we find a generative theoretician getting his hands dirty by stepping into the field of computational linguistics. Anderson correctly observes that there is rather a large mismatch between existing computational models of morphology (of which *Two-Level Morphology* with its embodiment in the family of KIMMO-systems is an important example) and what is theoretically required. Anderson then presents his own sketch of a plausible computational model of morphology, but which depends heavily on parallel processing and therefore for the time being he considers to be 'unimplementable'.

Anderson has the openness of mind to incorporate insights from a diversity of fields and schools of thought, whether they be traditional grammar, structuralism, different brands of generative grammar, descriptive linguistics or computational linguistics. All the more appropriate is the motto printed at the beginning of this book: 'Linguistics will become a science when linguists begin standing on one another's shoulders instead of on one another's toes'.

This work is not a basic morphology textbook. It presupposes familiarity with generative terminology and with generative rule systems: how they work and how they may be organized. Too often

the reader is referred to arguments or examples published elsewhere in the literature. One instance is on p. 44:

> Some examples of this difference in the interpretation of variables, which will not be repeated here, are cited in Anderson 1975.

The point of the subsection where this quotation occurs can remain obscure to the reader who does not have Anderson 1975 at hand.

In conclusion, much exciting food for thought can be found in Anderson's *A-Morphous morphology.* ■

Grammatical Voice. Cambridge Studies in Linguistics No. 59. By M. H. Klaiman. Cambridge: Cambridge University Press, 1991. Pp., xix, 323. Hardback. $64.95

Reviewed by Jean Baumbach
SIM, Niger Republic.

Grammatical Voice is a typological study of voice systems aimed at the linguist. It is based on a multi-language survey and is well written, but quite technical in approach. Klaiman states in the first chapter that the aim of his book is 'to clarify the understanding of grammatical voice and voice systems by advancing a typological scheme for their description' (p.xiv).

The book is divided into six chapters. The first, consisting of 44 pages, is an overview of various voice behaviors. Currently there are at least three distinctive ways in which the term voice is used. Klaiman proposes the division of these voice behaviors into three broad categories: derived; basic; and pragmatic. These may be described as follows:

1) Systems where regular alternations in verbal morphology signal alternate allocations of nominals among positions in structural configurations—derived voice or passivization;

2) The identification of voice with verbal oppositions which signal alternations in the participant roles of subject—basic voice, i.e. active and middle;

3) Systems in which the alternations of verbal morphology signal the alternating assignment among a clause's nominals of some pragmatic status or salience—pragmatic voice systems.

In chapter two (65 pages), Klaiman looks at active, middle, and passive voice divisions. He enumerates three different views of the functions of the middle voice. These functions include:

1) Signalling lowered transitivity;

2) Marking valence alternations, particularly valence reduction;

3) Relating middle and reflexive.

He examines each of these views in relation to the data found in Fula, Tamil, and Indo-European. Based on data from these languages, Klaiman concludes that both active and middle are basic, while passive is a derived voice.

Chapter three (50 pages) focuses on the relationship between control and voice. Klaiman begins by discussing the theta-hierarchy. This predicts which participant is most likely to be the logical subject of a predicate based on its role in relation to the verb, i.e. agent, undergoer, etc. In this chapter he also examines active-stative systems such as those illustrated by Chocho, an Uto-Aztecan language. In connection with active-stative systems, Klaiman discusses nominative/accusative versus ergative/absolutive case-marking systems.

In the second half of the book, Klaiman examines pragmatic voice systems. These are of two types: those driven by information structure salience; and those based on ontological salience, that is the referent's relative importance to the speaker and hearer. Klaiman looks at the latter in the fourth chapter (55 pages), where he describes direct and inverse systems. He contrasts configurational languages, e.g. English, with non-configurational ones, including Apachean, Tanoan, and Algonquian languages. Klaiman goes on to

compare the active-passive system with the direct-inverse system, identifying various traits of the latter type.

In chapter five (32 pages), Klaiman examines information salience voice systems found in both Mayan (Mam and K'ekchi) and Philippine (Cebuano and Tagalog) languages. Two types of information salience are discussed here: topic and focus. In Mayan languages the functions of this voice system include emphatic fronting, questioning and relativization. Whereas Mayan languages have both derived and pragmatic voice structures, Philippine languages have only the latter. In this chapter, Klaiman also looks at two types of anti-passive construction.

In the sixth chapter (eleven pages), Klaiman attempts to assimilate the information from the preceding chapters and to suggest directions for further study in formulating a theory of voice. He states that the different types of voice are sensitive to different levels of grammatical organization. These include: 1) the relational structure as in the active/middle/passive system; 2) the informational structure, as in the topic/focus system; and 3) the ontological structure, as seen in the direct/inverse system.

This book is definitely written for an audience with a strong background in linguistics. Many technical terms are used, each time followed by an asterisk. Klaiman generally explains a term when it is first introduced. One-and-a-half pages of abbreviations precede the first chapter. There is an extensive index and a bibliography consisting of approximately 260 entries. The latter reflects the wide survey of languages used for this book, as well as a thorough control of the literature on grammatical voice. There are twenty-three tables. Klaiman has numerous appropriate examples from various languages to support his conclusions.

I did not like the arrangement of the endnotes, which appeared following the last chapter, rather than at the end of each chapter, or better yet, the bottom of each page. This discourages the reader from taking note of them.

This is a text written for a serious inquirer into the topic of grammatical voice. It is a well-organized book, and the author's points are well illustrated and documented. He begins each chapter

by giving an overview of things to come, and summarizes each chapter, as well, which I found especially helpful. ■

A Grammar of Boumaa Fijian. By R. M. W. Dixon. The University of Chicago Press, 1988. 375 pp. Cloth $59.95, paper $26.95.

Reviewed by J. Albert Bickford
SIL-Mexico Branch and the University of North Dakota

This excellent grammar covers the Boumaa dialect of eastern Fijian, one of two closely-related languages indigenous to the Fiji Islands, and one which is mutually-intelligible with the Standard Fijian which has developed as a *lingua franca* since contact with Europeans in the 19th century. Its importance derives both from its presentation of a local dialect previously under-represented in the literature on Fijian, and more generally as an example of how to write a descriptive grammar, with a wealth of information in a readily accessible format and abundant examples.

There are two initial background chapters, one covering sociolinguistic issues such as language use and related languages, the other surveying the phonology. At the end are three texts (two narratives, each about 15 minutes long when originally recorded, and a short hortatory village announcement), with morpheme glosses and sentence translations. The book also includes three maps, a review of previous work on Fijian, and a vocabulary of about 1000 items.

The core of the grammar starts with three overview chapters covering general word structure, general syntax, and the syntax and morphology of verbs. Twenty more chapters follow covering all the standard topics, plus quite a few others that are often omitted from descriptive grammars (the number system, nominalized NPs, interjections, comparative forms of adjectives), and many that illustrate Fijian's uniqueness, especially the meaning and behavior of specific morphemes. Dixon has allowed the outline of the book, as well as the specific claims made in it, to arise out of the data. Throughout there is enough detail to give a rich sense of how

Boumaa Fijian is different from all other languages (including other varieties of Fijian), yet presented in a way that draws out similarities.

This organization, with an overview of the whole system first and details later, shows sensitivity to the needs of a reader encountering the language for the first time. Dixon has avoided the mistake too often made in reference grammars, organizing the grammar according to some *a priori* scheme (a cross-linguistic outline or theoretical perspective). Of course there is a trade-off here; using a language-specific approach to organization may make a book more difficult to use for occasional reference, when readers will want to find things according to standard cross-linguistic categories. For example, Dixon discusses verb morphology in at least three chapters with passing references in other places. Each chapter gives different details and a different perspective, and someone interested in some particular aspect of verb morphology might not find all relevant information in one place. Chapters sometimes need to be placed in a sequence which lacks an obvious rationale, at least until one understands the structure of the language. Again, finding the information you want may be impeded.

The book unfortunately lacks an index to facilitate its use as a reference, but to compensate for it the table of contents is quite detailed, the terminology used is for the most part standard (so it's easy to recognize when you've found what you're looking for), there is extensive use of cross-references throughout the text, and at key places, a road map about how to find one's way through the book. I tried looking up several different topics, and was able to find useful information quickly about all except one. (I eventually stumbled onto it. It was clearly listed in the table of contents but in an unexpected place.)

Dixon's appreciation for the language and its speakers is evident throughout. He acknowledges the help of individuals by name and with specific mention of their skills and contributions. About one man, Josefa Cookanacagi (Sepo), he says (p. xiii):

> He had thought about language all his life, and now shared with me the insights he had attained... He [read a draft of the entire book and] sent me a notebook with comments and corrections on every chapter, and on the texts and vocabulary. This is, in a real sense, Sepo's grammar.

Such open respect for the people among whom we as linguists work is sadly not expressed as often as it should be, although the practice in our profession is improving. We would all profit from imitating Dixon's example.

Indeed, his book contains much which should be emulated in all descriptive grammars. He bases his conclusions primarily on a corpus of texts and his personal experiences interacting as a speaker within the community, and draws on elicited material only when necessary to clarify issues that arise out of the natural data. Statements about the language are backed up with examples and, in some cases, with arguments that draw in other relevant facts.

The treatment is in general pretheoretical (and thus comprehensible to a wide range of readers), but frequently shows sensitivity to issues that are important in the theoretical literature. For example, in a discussion (p. 26) of certain verb roots which have long vowels when occurring by themselves, but short vowels when suffixed, he cites one type of reduplication to argue that the vowels are underlyingly long. While considering basic constituent order (pp. 242-244), he first points out why this is largely a non-issue in Fijian due to the rarity in texts of clauses that contain both subject and object, then cites what evidence there is (from elicitation) that the basic order is VOS.

I do not always agree with the analysis (for example, the definition of 'affix' on p. 27 which encompasses some elements that are separate words, according to the phonological criteria spelled out on pp. 24-25), but in these cases the facts are presented clearly and in enough detail that an approach from another direction is possible.

The most serious difficulty I had was that the book seems to expect too much of my ability to learn Fijian. Many examples are quoted within a paragraph rather than being set apart with morpheme glosses; sometimes these have a free gloss, sometimes not even that much help is provided. This may reflect an attempt to save space or increase the smoothness of the prose under the assumption that more extensive glossing is not necessary. I regard this economy as misplaced; many times I found myself looking around trying to piece together a gloss because I had not yet absorbed the vocabulary. This interferes with comprehension of the whole, and is a blemish on an otherwise very clear presentation of the facts. The problem would be especially acute for someone who wanted to use the book only for

occasional reference. As a rule of thumb, I'd suggest that all examples in discussions about a language which is not familiar to one's readership should have morpheme glosses whenever they occur, with a possible exception when the same item is mentioned several times within one paragraph.

Another practice which comes across as expecting too much of the reader is the occasional use of a morpheme gloss based on a morpheme's category rather than its meaning. For example, all aspect markers are glossed 'ASP' (pp. 76ff.), requiring a reader to remember what each marker means or constantly refer back to the section that describes them.

Overall, this is an admirable grammar—very helpful in giving a clear and comprehensive picture about this dialect of Fijian to the extent these goals are ever possible. The few (and probably inevitable) shortcomings do not diminish the value of the book; there are so many things done well. This is one of the best descriptive grammars available and well worth consulting, if for no other reason than to serve as a model for grammars in other languages. ■

22nd International Systemic Functional Congress

July 19-24, 1995; Department of English, Peking University. For further information contact Prof. Hu Zhuanglin, Dept. of English, Peking University, Beijing, 100871, China.

23rd International Systemic Functional Congress

July 15-19, 1996; Centre for Language and Literacy, School of Adult and Language Education, Faculty of Education, University of Technology, Sydney, PO Box 123, Broadway 2007, New South Wales, Australia. For further information contact Ms. Diana Slade, Email address: D.Slade@UTS.EDU.AU

Sociolinguistic implications of academic writing. By E. A. Nida. *Language in Society*, 21:477-485.

Reviewed by Patricia M. Davis
SIL—Peru Branch

SUMMARY

Although knowledge gained from research needs to be widely disseminated, the language used in many academic journal articles is so technical that only specialists are able to understand it. Nida views this trend as doubly detrimental. First, well-educated native speakers, including graduate students, are discouraged from reading and thereby acquiring the information. Second, large numbers of scholars in developing countries who desperately need the knowledge in order to keep abreast academically cannot access it because of the high English-language competence required. Illustrating from articles which appeared in *Language* and the *American Anthropologist*, Nida identifies three major areas of difficulty: vocabulary, complexity of syntax, and organization of content.

1. **Vocabulary**

   Technical language tends to be a special register—a type of cognitive shorthand—which enables specialists to express ideas concisely and also signals 'in-group' belonging. However, if an article contains an overload of specialized vocabulary, readers tend to skim, gathering terms but not a good comprehension of the subject. Among the items which cause problems:
   a. Newly coined, or unnecessarily complex terms, e.g. passivization, unaccusative.
   b. Ordinary words given unusual meanings, e.g., 'a clitic which **realizes** quantified NPs', rather than the clearer 'a clitic which marks noun phrases having quantifiers'.
   c. Overload of high-level vocabulary, e.g. superorganic, comparandum.

2. **Complexity of Syntax**

   a. Phrases consisting of unusual combinations of words, e.g. 'inchoative verb serialization'.
   b. Long series of modifiers. Five prepositional phrases in a

series is too much, says Nida, but it is easier to understand those formed to the right of the head noun (e.g., 'a key dimension of the human mind in its natural habitat: on the midway of social life') than those formed to the left of the head noun (e.g. 'culturally organized experiential schemata').

c. Redundancy, e.g., 'collective representations as transpersonal'. (Collective representations **are** transpersonal.)
d. Complicated generic terms, e.g., 'Socially mediated cultural manifestations' is a heavy way to refer to ritual dances.
e. Unusual use of adverbs that do not signal quantities, qualities, or degrees, e.g., 'inanimates inflect ergatively'.

3. **Organization of Content**

a. Inclusion of material in parenthesis which disrupts thought with contrary opinions, irrelevant additions, and similar unrelated material.
b. Long, complex sentences, especially those which contain several subparts.
c. Over use of abbreviations and acronyms.
d. Bibliographical references in the text, if they are used more as name-dropping devices rather than as serious citations.
e. Heavy use of footnotes (more than 25 percent of the page space is too much).
f. Summaries which cover too much of the content in such highly generic and hard-to-understand language that readers tend to give up on the article.
g. Inappropriate appendices. Nida feels that appendices should consist mainly of statistical data and charts.
h. Material of lesser import. Help the reader by signaling it with smaller type.
i. 'Mathematical-like' formulas in social science writings do not necessarily make meanings more true or more intelligible.

Nida observes that use of highly specialized language often reflects elitism. Scholars write for their peers in professional dialect; journals therefore become increasingly specialized. A radical shift in social sensitivity is needed if anyone outside of the 'in-group' is to profit. Nida suggests scholars learn to write on two different levels: technical and semi-popular.

USEFULNESS

This article relates directly to the role of language in knowledge acquisition. First, Nida identifies an academic trend toward highly-specialized language use which is detrimental to the diffusion of research knowledge. By listing specific areas of problem, with examples, he makes it easier for scholars to edit and to clarify works intended for publication. He analyzes the psychological and sociological milieu of our day, enabling readers to understand why the use of obscure technical jargon has become so popular even though many do not understand it well. Finally, rather than cudgel his peers, he proposes a compromise (use of both technical and semi-popular styles), seeking to accommodate the elite specialists in each field as well as learners and those less knowledgeable.

CRITIQUE

Nida has targeted a problem often decried by English teachers, graduate students, and the general public. What makes this article different and more helpful, is the specific listing of forms which obscure meaning coupled with suggestions as to how the wording might be clarified. Too often scholars succumb to peer pressure and the sense of power achieved by the use of technical jargon, forgetting the larger segment of the audience with whom they need to communicate. Nida's insightful motivational analysis, which rings true in experience, makes us aware of those shaping influences and gives us reason to resist them, at least at certain times. Happily, his writing exemplifies the qualities he espouses despite an occasional new term like 'mathematical-like' (p. 483).

The article is a call to express knowledge so clearly that it is accessible to all. As scholars we should enshrine a copy on the bulletin board by our desks. ■

Stellenbosch papers in linguistics Nos. 17, 20-24 (SPIL). By Rudolf P. Botha, ed. University of Stellenbosch. 1988, 1989-91.

Reviewed by Karl Franklin
South Pacific SIL, Australia

The SPIL series is published at the University of Stellenbosch in South Africa by the Department of General Linguistics. Most of the contributions are in the form of working papers. To give some idea of SPIL's usefulness I will briefly review the contents of six issues, between the years of 1988 and 1991 (the latest that was on hand).

SPIL No. 17 (1988)
A face for the future. Pp. 121
This is a collection of four papers to mark the tenth anniversary of SPIL.

The first papers is a translation of an article in Dutch by Jan B. Bedaux called 'The portraits of Simon Van Der Stel, first governor of the cape', and its significance lies in documenting early inter-ethnic marriage in South Africa.

The second papers, by Botha, is 'Semantic evidence against the autonomy of the lexicon'. The evidence is from Afrikaans and Xhosa.

The third paper is by Roger Lass and is called 'How to do things with junk: Exaptation in language evolution'. It addresses the problem of very old forms retained in English verb morphology.

The final article, 'Rules of conceptual well-formedness and optional vs. obligatory iterativity' is by Melinda Sinclair. It discusses certain frequency adverbials as being interpretation as either semelfactives (single occurrences) or iteratives (repeated occurrences).

SPIL Issues Nos. 20-24 (1989-91)
The metaphysics market.
by Rudolf P. Botha

Issue 20 (**Merchandizing language as matter**, 56 pp.) is a critical historical examination of the Bloomfieldian position on mentalism. Botha's style is to write personal short and somewhat witty

introductions and conclusions and intersperse them with serious criticisms of how Bloomfield and his followers (the Neo-Bloomfieldians) were not scientifically or philosophically respectable in their views of language.

Issue 21 (**Billing language as behavioural**, 80 pp.) is a criticism of linguistic and philosophical behaviourism, beginning with Pike and other Post-Bloomfieldians and extending to Chomsky's well known examination of Skinner. Botha sees the philosopher linguist Wittgenstein as having an oversimplified view of language. Others who are critiqued by Botha include Quine, J. R. Firth, Malinowski, and Halliday. None can account for the creative aspect of language.

In Issue 22 (**Selling language as soul**, 109 pp.) Botha uses religious metaphors and turns his attention (or vindictives) upon Chomsky: Externalized and internalized grammars, Universal grammar, abstract grammar, and the resulting but competitive conceptual grammars of Katz and Foder.

Issue 23 (**Pushing language as Platonic (not to mention Popperian**, pp. 84) examines the history of grammatical/linguistic universals as far back as Plato and as recently as Katz and Postal. These are real properties of the sentences of 'natural languages' which follow logical laws. Botha feels that Katz and Postal in particular have not accounted for the knowledge of abstract objects. He contrasts Plato's world of intelligibles (abstractions) with that of Popper's second (subjective) and third (objective) worlds. Finally, he disputes criticisms which Carr had made of Chomsky.

Issue 24 (**Stocking language as social stuff**) is the fifth in the series 'in which the prototypical conceptions of language are turned inside out'. In it Botha examines the works of Saussure, Labov, Sapir, Dummett, Papteman and Itkonen. Both the materialist and idealist characterizations of the nature of social reality are taken into account.

The SPIL series, notably the five issues on 'The metaphysics market' by Botha would make interesting reading for any course dealing with the history of linguistics. Aside from this rather restricted audience of graduate students or researchers, the articles seem of limited value for a typical SIL-field entity library. ■

A Field Guide for Sign Language Research.
By William Stokoe and Rolf Kuschel. 1979.
Silver Spring, MD: Linstok Press, Inc. 32 pp. $4.95.

Reviewed by Barbara F. Grimes
SIL-International Programs

One of the earliest linguists to describe a deaf sign language in linguistic terms from the field was James Kakumasu of the Summer Institute of Linguistics, writing about the sign language used by the Urubu Kaapor Indians in Brazil in 1968.

Most of the sign languages hearing people know about are those which have been described by linguists since 1960. Oliver Sacks (1988) says William Stokoe was probably the first linguist to 'really confront... the reality of Sign' (Sacks, p. 140). In 1955 Stokoe went to Gallaudet University in Washington, D. C., the only liberal arts university for deaf people in the world. In 1960 he published a '"bombshell" paper on Sign Language Structure, the first-ever serious and scientific attention paid to "the visual communication system of the American deaf"' (Sacks, p. 140). In 1965 he published *A Dictionary of American Sign Language*. It was 'the first description of the social and cultural characteristics of deaf people who used American Sign Language' (Sacks, p. 141). He has since published many other studies on deaf sign language and American Sign Language.

Rolf Kuschel has published a description of the sign language used on Rennell Island in the Solomon Islands (1973).

Gallaudet University's *Encyclopedia of Deaf People and Deafness* (1987) lists about 50 deaf sign languages. The 1992 *Ethnologue* lists an additional 30. It has been suggested that there may be hundreds more deaf sign languages used in the world, not counting possibly thousands of 'home sign' languages used in small family groups. Much more research is needed to find and describe other sign languages. The *Field Guide For Sign Language Research* by Stokoe and Kuschel is a useful handbook for linguists and other investigators wanting to describe such sign languages.

In their introduction, the authors discuss sign language systems, sign language versus kinesics, and sign language as a language.

Under methodology they discuss problems of translating, and interpreters.

The third section covers making records: technology, notebook, still photography, motion picture photography, videotape, and general cautions.

The last section deals with general topics: ethics, questionnaires on general social information, signs, basic sign vocabulary, checklist of cultural items, sentences for sign translation, first steps in analysis, archives, outlets, and references.

The entire *Field Guide* is only 32 pages long . Anyone interested in describing and documenting a sign language should start with Sacks' book and this guide by Stokoe and Kuschel. All the references cited in this review contain bibliographies which can lead an investigator to other references documenting specific sign languages.

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Courses on Linguistics Software Offered

JAARS Computer Services, Waxhaw, NC offers short courses on software used for linguistic analysis and data management, as well as for adaptation of text between dialects or closely related languages.

Language Applications: Teaches Shoebox 2.0, CC, TA/SRT, Shapes and CECIL. Assumes working knowledge of a word processor, MS-DOS, and Standard Format. Course Fee $65 for SIL members, $135 others. Course—Apr 18-29, 1994, Workshop—May 2-6, 1994; or Course—Oct 31-Nov 15, 1994, Workshop—Nov 16-22, 1994.

CARLA (Computer Assisted Related Language Adaptation): Introduces AMPLE (A Morphological Parser for Linguistic Exploration) and STAMP (Synthesis and Transfer for AMPLE), and related programs used in adapting text between dialects or closely-related languages, as well as in producing interlinear text by means of a more powerful morphological analyzer than Shoebox. A following workshop is designed for persons with language materials in hand ready to begin using CARLA. Course Fee: $35 for SIL members, $45 for others. Course—Jan 24-28, 1994, Workshop—Jan 31-Feb 11, 1994; or Course—Sept 19-21, 1994, Workshop—Sept 26-Oct 5, 1994. An additional CARLA course may be scheduled for Dallas in late May 1994 at the International Linguistics Center.

Software and Manuals for both courses are included in the fees. For further information contact: JAARS, ICS Computer Training, Box 248, Waxhaw NC 28173-0248. Phone (704) 843-6151.

Oregon SIL—June 13-August 12, 1994

Each summer SIL Oregon holds a workshop in grammatical description. It is designed for experienced field linguists interested in developing an insightful and clear grammatical description using a functional and typological approach. This approach focuses on non-formal description of grammatical structures as they function in a discourse and communicative context. Participants take syntax and semantics courses for credit or as auditors to help them shape their grammatical description. SIL faculty provide consultation on specific language problems with the eventual production of a publishable grammar sketch as the goal.

Prospective participants should have at least two years experience in investigating a language, and should come prepared with interlinear texts of several genre. If interested request a 1993-94 SIL Course Catalog and Application Form from the Admissions Office, 7500 W. Camp Wisdom Rd., Dallas, TX 75236 USA.

North Dakota—SIL June 13-August 12, 1994

During the summer of 1994 SIL North Dakota will offer courses customized to meet the needs of linguists, field workers, and graduate students. A number of courses that can apply for credits are available. Included will be Computerized Parsing (an introduction to computerized morphological and syntactic parsing with application to related language adaptation) and Analysis and Description of Field Data (especially recommended for folks who are apprehensive about writing, it will include specialized instruction in writing for a technical audience, how to use writing as a research tool, how to interact with comments from colleagues, and how to develop ideas; participants should bring data, ideas, and drafts to use as raw material for this course.

For more information, write for the 1994-95 SIL Course Catalog, SIL Admissions Office, 7500 W. Camp Wisdom Rd., Dallas, TX 75236; or contact Steve Marlett, Box 8987 CRB, Tucson, AZ 85738; e-mail: steve.marlett@sil.org.

XIIIth International Congress of Phonetic Sciences (ICPhS 95)
Stockholm University, Stockholm, Sweden
August 13-19, 1995

Meetings will include a mixed format of plenary and semi-plenary lectures, topical workshops and symposia, poster sessions and parallel oral sessions. Participants to suggest topics for such events. Technical exhibitions and book exhibitions will be arranged as part of the congress. The official language will be English.

Preliminary dates and deadlines:

September 1994—Call for participation/abstracts
November 1994—Deadline for submission of abstracts
January 1995—Notification of acceptance; Regis. deadline for lower fees
April 1995—Deadline for full paper
May 1995—Distribution of congress program

Write: Congress Secretariat; ICPhS 95, c/o Congrex; P. O. Box 5619; S-106 91 Stockholm, Sweden. Ph: +46-8-612 69 00, Fax: +46-8-612 62 92. Email: congrex@ask.se.

Scientific Secretariat; ICPhS 95, Department of Linguistics; Stockholm University, S-106 91 Sotckholm, Sweden. Ph: +46-8-16 23 47, Fax: +46-8-15 53 89, Email: icphs95@speech.kth.se

Assistance Requested of Linguists working in South East Asia/ Melanesia—Past & Present. I am a research fellow in the Dept. of Linguistics at University of Melbourne, Australia, employed on a large-scale typological survey of the sound systems of South East Asia and Melanesia. In order to make the survey as fully comprehensive as possible I am desperately trying to track down copies of unpublished phonological descriptions for possible use. SIL linguists have written hundreds of such descriptions as a result of their fieldwork and expertise in phonemic analysis. I ask these linguists (many of whom can no longer be traced by SIL branches in the area, especially those working in the 60s, 70s and 80s) to contact me as soon as possible. I would appreciate it if linguists could send copies of their descriptions directly to me. Any other help and correspondence would be appreciated. Contact direct: +61 3 347 7305 (fax); +61 3 344 5191 (work) or +61 3 489 5583 (home); john_hajek@muwayf.unimelb.edu.au. Many SIL linguists have been of enormous help. May others contribute as well. **Dr. John Hajek; Dept. of Linguistics; Univ. of Melbourne; Parkville 3052; Melbourne, Australia**

Books Available For Review

The following books are available for review by our readers. If you wish to do a book review for publication in *Notes on Linguistics*, contact the editor, and the book will be shipped to you along with instructions for submitting the review. When you submit a review, the book is yours to keep. Contact:

Notes on Linguistics
Attn: David Payne, Linguistics Coordinator
7500 West Camp Wisdom Road
Dallas, TX 75236

Alexeal, Melissa. 1993. The semantics of time: Aspectual categorization in Koyukon Athabaskan. Lincoln, Nebraska: University of Nebraska Press. 199 pp. Cloth

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30th Regional Meeting of the Chicago Linguistic Society—April 14-16, 1994

The special parasession will cover the treatment of variation, which has proved to be a thorny issue in both synchronic and diachronic linguistics. **Invited Speakers:** Joan W. Bresnan, Stanford University; Gregory R. Guy, York University; Richard Kayne, CUNY; Anthony Kroch, University of Pennsylvania; Salikoko S. Mufwene, University of Chicago; Janet Pierrehumbert, Northwestern University; Gillian Sankoff, University of Pennsylvania.

Deadline for abstracts (January 15, 1994) was past before scheduled publication of this issue of *NL*. For information or to get on e-mail list: cls@sapir.uchigago.edu (312) 702-8529.

ERRATA

The following corections were received from Dr. Ozo-Mekuri Ndimele re: *Intra-Clausal Movement as a Response to Case Summon*, published in *Notes on Linguistics No. 62*:

Page 16 (diagram) add subscript *s* to AGR = AGR_s

Page 17 (line 5) change upper case S in VS to lower case = Vs

Page 25 (title 2.4) change AGR_s'' to AGR_o''.

The following corrections were received from John Roberts re: *Mirror-image reduplication in Amele* published in *Notes on Linguistics No. 63*:

Page 29 (first full paragraph, last two sentences): ...except the *nuʔun* case. ...a suffix, such as *-ʔVn*, attaching to these forms. [The glottal did not print.]

Page 30 (Table 4, 5th vowel change):

filihiʔdoʔ	'to unravel something'	filihiʔ-folohoʔdoʔ	'to unravel something all over'

Page 31 (1st paragraph, line 3): ...interrogative pronoun *in* 'who(sg)' reduplicates to *inni* 'who(pl) is it'. [*in* was not italicized in *NL*.]

The following corrections were received from Regina Blass re: *Response to E. Lou Hohulin's Review of Her Book, Relevance relations in discourse: A study with special reference to Sissala*, published in *Notes on Linguistics No. 63*:

Page 36 (4th paragraph, line 7): change 'assumptions in memory' to 'assumption drawn from the cognitive environment'.

Page 37: (End of paragraph): change to 'assumptions drawn from the cognitive environment'.

Page 43 (2nd paragraph, line 7): change 'contextual fact' to 'contextual effect'.

Page 43 (3rd paragraph, line 10): change 'to be eliminated' to 'to eliminate another'.

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From the Linguistics Coordinator

A major focus of the Academic Affairs section of SIL for 1994 concerns 'Consultant Corps Development'. Since the beginning of the year I've been chairing a committee composed of the SIL International Coordinators for Anthropology, Linguistics, Literacy and Translation, working to assess the current situation regarding consultant help around the world of SIL, and to formulate our vision of an ideal situation for five to ten years down the road.

We've moved on to propose strategies for moving toward this ideal, as well as some specific steps to help get the various entities of SIL there. We're considering consultation in general, across the academic domains represented in SIL (anthropology, linguistics, literacy, translation), rather than treating each domain in isolation.

In addressing this, we think we are dealing with something that SIL field teams and entities consider to be a pressing issue. At least it is something we hear voiced a lot. We will want to interact with many of you—field linguists, consultants, administrators—to make sure we are addressing *your* concerns in the area of consultant help, as well as to get *your* ideas for how best to move our organization toward the envisioned ideal.

We're proposing several mechanisms for getting broad-based input on this issue. But one I'd like to pursue now is to invite you to write me any of your thoughts on consultant help in SIL. Ideas for improving the organization in this area will be particularly welcome.

Although our committee has been dealing with consultant issues in general —not those peculiar to a specific domain, I will mention here two areas I consider to be the most important ones currently in linguistics: (1) Helping field teams produce reference grammars that are well-accepted by the linguistics community, and (2) Assisting field teams in doing computer-aided parsing and testing of analyses (i.e. formulating and testing formal grammar fragments). These are ones for which we need to find better ways to provide adequate consultant help.

Let me know if you have any comments!

—David Payne

Validity and reliability in language survey testing

Barbara F. Grimes
SIL—International Programs

This paper focuses on the importance of validity and reliability in the kinds of testing that are needed to determine several things about language relationships and usage: (1) how closely related two language varieties are, (2) how well speakers of two related language varieties understand each other without having learned some of the other variety, and (3) how proficient speakers of one language are in a second language with which they have had contact. It discusses the most direct testing methods available to date, and shows how modifications to those methods or substitution of other methods may lead to unreliable and invalid results. The paper is written from the perspective of surveys in isolated, preliterate, minority language situations. Statistical methods to test validity and reliability of specific tests are not covered.[1]

1. Validity and Reliability. Hatch and Farhady (1982:254) state:

> Both reliability and validity are of critical importance if you wish to convince others that your findings and your interpretation of the findings are credible.

Fasold (1984:90) points out that validity is stricter than reliability. If a measure is not reliable, it cannot be valid; that is, if repeated testing with that measure yields different results, then the method cannot be measuring what it claims to measure in an accurate way. But if a measure is reliable, that in itself does not prove that it is valid. It may give the same results every time, but be based on false assumptions about the relationship between that particular test and the question under investigation.

1.1 Reliability. Hatch and Farhady (1982:244) define *reliability* as 'the extent to which a test produces consistent results when administered under

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[1] This paper is a revision of one of the same name presented at the First Summer Institute of Linguistics Language Assessment Conference held at Horsleys Green, Bucks., England in May 1989.

similar conditions'. Unreliable test scores result when a test design produces inconsistent results. In addition, scores may be unreliable if there are errors made in measuring them, if the subjects experience fatigue during testing, or if there are problems in the test setting such as noise or interruptions.

Factors that improve reliability are (1) longer length of test, (2) homogeneity of items, (3) power of test items to discriminate among subjects, (4) wide variability of group ability, and (5) sufficient test-taking time, which is the most important (p. 250).

Hatch and Farhady (p. 246) caution their readers that:

> If you run your experiment and never report the test reliability, we have no way of knowing whether it has this basic requirement of a good test. We can judge the appropriateness of the design and we can judge the appropriateness of the situational procedures used in the study, but we will not know whether your results are meaningful unless you also report on the reliability of the test itself.

Reliability can be estimated by at least three basic methods (p. 246):

1. *Test-retest.* The test is administered to the same subjects twice and the results compared.

2. *Parallel tests.* Parallel tests are administered and compared. Parallel tests have equal variance, equal covariance,[2] and equal correlations with another criterion.

3. *Internal consistency methods.* Tests are split into two equivalent halves and each half compared with the other for consistency, or the consistency of pairs of items is compared.

Wilson (1952:46) emphasizes that *replication* (test-retest) is especially necessary when the class under study is not too precisely defined and is subject to wide individual variations. On the other hand, 'if the material is sufficiently uniform but the measurement lacks precision, replications may

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[2] **Variance**: the mean of the squares of the variations from the mean of a frequency distribution. **Covariance**: the expected value of the product of the deviations of corresponding values of two variables from their respective means.

or may not increase the accuracy of the result'. This would be true of using lexicostatistics to measure genetic linguistic relationship, and therefore inherent intelligibility; the measure is imprecise no matter what, so retesting is meaningless.

1.2. Validity. Hatch and Farhady (1982:250-251) define *validity* as 'the extent to which the results of the procedure serve the uses for which they were intended'. Validity refers to the results of a test rather than to the test itself. Validity can be high, moderate, or low. It is dependent on the use of the results. 'A test can be highly valid for one purpose but not for another' (p. 251).

Three kinds of validity are especially relevant for language survey testing: *content validity, criterion-related validity,* and *construct validity. Content validity* has to do with 'the extent to which a test measures a representative sample of the subject matter content' (p. 251). It looks at the adequacy of the content sample, therefore, the content of what we want to measure must be carefully defined. If we wish to measure how closely related two language varieties are to each other, we need to understand what genetic linguistic relationship entails and what kind of language samples will be needed to make adequate comparison. If we wish to test inherent intelligibility, we need to understand what inherent intelligibility entails, how closely related the language varieties are, and what kind of language sample will be needed to adequately evaluate that inherent intelligibility. If we wish to test bilingual proficiency of speakers of one language in their second language, we need to understand what bilingual proficiency entails, what the relationship between the two languages is, if any, and what kind of language sample will be needed to adequately evaluate that bilingual proficiency. That is *content validity.*

Criterion-related validity has to do with using test scores 'to predict future performance or to estimate current performance on some valued measure other than the test itself'. If we are using a test to try to predict what performance on the Second Language Oral Proficiency Evaluation (SLOPE) would be, that has to do with criterion-related validity.[3] In order to do that we need to compare the results of that test with SLOPE enough times and in enough situations to find if they are valid. If we are using a test to predict future performance, such as language performance in school,

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[3] SLOPE is further described in Section 5 of this article.

in U. S. Foreign Service Institute (FSI) employment, or in use of Scripture in a second language, that is criterion-related, or predictive validity (p. 251). To do that we need to correlate the results with a valid test which measures what we want to know. Hatch and Farhady point out that it is possible for two tests to correlate highly in criterion-related validity but for neither test to be valid. It is necessary for one of them to be recognized by qualified scholars as being valid for the purpose intended (p. 254).

Construct validity has to do with interpreting test performance in terms of psychological or psycholinguistic traits (pp. 252-253).

Statisticians also distinguish between *internal validity* and *external validity* of a research study. *Internal validity* is 'the extent to which the outcome is a factor you have selected rather than other factors you haven't controlled'. If we use a test for inherent intelligibility to test subjects who have enhanced their understanding of a related dialect through second language learning, the results will be inflated by a factor we have not controlled and the internal validity of the test will be damaged. A careful study considers the factors that weaken internal validity and seeks to eliminate their influence (pp. 7-8).

External validity has to do with 'the extent that the outcome of any research study would apply to other similar situations in the world'. A highly controlled situation for a pilot study may produce a test that is unusable on the field. For example, including bilinguals in an intelligibility test sample may not be misleading in the relatively few situations in the world where bilingualism is fairly uniform but it is not useful as a general model. External validity also has to do with whether the population sample is truly representative of the whole population. For example, using a small population sample of ten will not be large enough for testing bilingualism because of different proficiency in different segments of the test population. Procedures must be restricted as carefully as possible in order to produce the most valid results (pp. 8-9).

Factors that can influence validity negatively are (1) unclear directions to test subjects, (2) difficult vocabulary or syntax, (3) inappropriate level of difficulty of test items, (4) poorly constructed test items, (5) ambiguity, (6) test items inappropriate for the test purpose, (7) not enough test items for test objectives, (8) improper arrangement of items, (9) identifiable patterns of answers so that guessing is possible. Other factors that can influence validity are the care used in test administration procedures and in interpreting the subjects' responses, and differences within the group of

subjects, such as culture, that could influence results or cause misunderstanding (p. 253).

2. Directness and Objectivity of Measures.[4] Figure 1 shows the possible relationships between directness of testing, and objectivity and bias in the possible responses of the subject and the evaluation of the investigator. A test instrument which is designed to test directly the question under investigation is expected to produce valid results. Repeated testing by that method is expected to produce the same or equivalent results, and those results should agree with other expert knowledge of that situation and of similar situations (external verification, Lieberson 1967:144-150).

If it is not possible to test directly but only indirectly, then the indirect test results need to be calibrated against the results of direct testing in a pilot study (criterion-related validity). The question under investigation in an indirect test should have a logical or demonstrated relationship to the main question under investigation. Fasold (1984:90) gives as an example an attempt to measure intelligence by measuring foreheads. The results could be reliable—a person's forehead always measures the same—but they would not be valid.

The investigator needs to understand the limitations of the relationship between the two questions and to give adequate consideration to those limitations in his evaluation. If it is possible to test only part of the question under investigation, the investigator needs to understand the relationship of that part to the whole and to give adequate consideration to that limitation. Any additional variables introduced into the results by the limitations of indirect or incomplete testing need to be weighed objectively against any supposed advantages in time or effort that kind of limited testing may seem to afford.

It is sometimes thought that what have been called 'objective' or closed-ended test methods produce accurate results, whereas what have been called

---

[4] **Direct**: straightforward, without intervening conditions. **Indirect**: not straight to the point, secondary, oblique, circuitous, roundabout. **Objective**: uninfluenced by emotion, surmise, or personal prejudice, based on observable phenomena and agreed upon criteria, presented factually. **Subjective**: dependent on criteria that are not generally agreed upon or accessible. **Biased**: with preference or inclination that inhibits impartial judgment, prejudice. **Unbiased**: without bias or prejudice, impartial.

'subjective' or open-ended tests may not. However there are various points in an evaluation process where subjectivity and bias may enter. The bias may be conscious or not (Huff 1954:123). It is possible to have a closed-ended testing instrument but to have bias entering into the subject's answer or the investigator's evaluation. It is also possible to have an open-ended test where bias does not introduce inaccuracy.

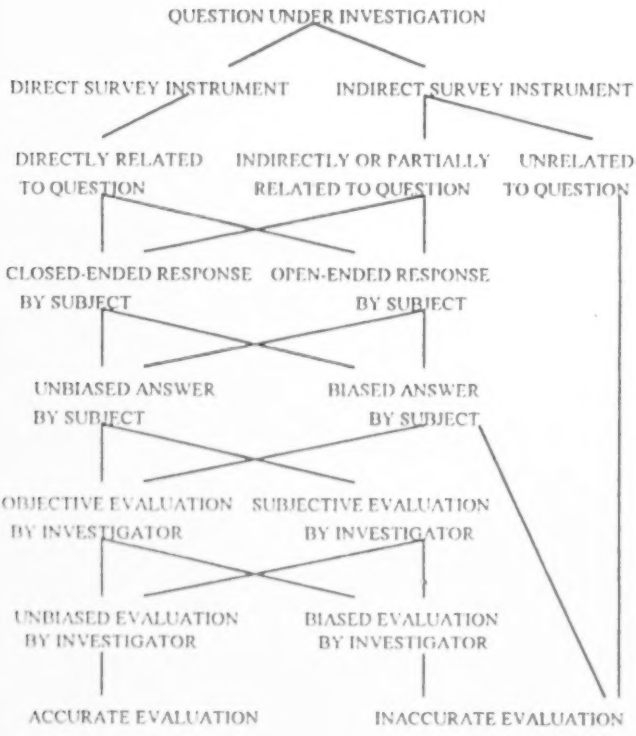

FIGURE 1. DIRECTNESS AND OBJECTIVITY IN SURVEY TESTING

3. Evaluating Closeness of Genetic Relationship. Comparative linguists are interested in historical processes and the resulting language classification. The *comparative method* primarily demonstrates the systematic phonological changes among language varieties that have diverged from a common ancestor. It is the most direct and valid method for demonstrating genetic relationships, and thus arriving at language classification. Comparative linguists frequently investigate lexical and syntactic relationships between languages and dialects as well, in addition to phonological change.

Morris Swadesh (1952) proposed what he called 'lexico-statistics' as a faster way to get at relationships than was needed at that time to carry out the comparative method. The method compares assumed cognates between two related language varieties. Swadesh assumed that language change takes place at a uniform average rate within all languages He attempted to calculate the time depth since divergence between related varieties by applying lexicostatistics to a time index, resulting in what he called 'glottochronology'.

Most other linguists have repudiated the idea that there is a regular relationship between language change and time, but some linguists have used lexicostatistics as a means of making a quick approximate comparison between related languages. B. F. Grimes (1988a:47-51) describes in detail the kinds of problems that go along with using this method as a measure of genetic relationship, so that will not be repeated here. Most linguists recognize that the method is inaccurate and documented cases are appearing in the literature to show how some of them have been misled by using this method.

For example, J. Sneddon (ms.) showed that Lolak of north Sulawesi, Indonesia, has 79 percent lexical similarity with Mongondow, 66 percent similarity with Ponosakan which is in the Mongondowic group, but only 63 percent with Kaidipang which is in the Gorontalic group. However, when he compared the structural similarities of Lolak with the Gorontalic languages and the Mongondowic languages, it became clear that Lolak is really in the Gorontalic group. The lexical similarity with Mongondowic languages is due to borrowing from Mongondowic languages which surround it.

Bernard Comrie (1990) found that the Haruai language of Papua New Guinea has 37 percent lexical similarity with Aramo which is in the Piawi group, and 35 percent with Kobon in the Kalam-Kobon group. Haruai had

previously been considered to be in the Kalam-Kobon group but an investigation of morphological structure showed that it is really in the Piawi group. The lexical similarity with Kobon is due to borrowing, and he found that lexical borrowing from Kobon is heavier in the northern region than elsewhere.

Ronald Sim (1988) found that although lexicostatistics suggest that Kambatta and Hadiyya of Ethiopia are no closer to each other than they are to the Sidamo language, the comparative method clearly shows that in terms of shared innovations, Kambatta and Hadiyya are closer to each other than either is to Sidamo.

M. L. Bender (ms.) says that Aka (Sillok), Kelo (Tornasi), and Molo of Sudan were classified by Evans-Pritchard as 'three Berta languages' on the basis of a few nouns. However, Bender says, 'Berta influence is strong and shows up even in basic vocabulary, but further vocabulary and grammar place these varieties with Gaam [Tabi]'. Gaam is in the Jebel group, not the Berta group.

Lexicostatistics is only an indirect measure of language relationships. It measures assumed cognates, and because these often include borrowings it may introduce invalid data into the calculation. It also does not compare grammatical or systematic phonological relationships. It is weak in content validity. It can be used as a first approximation to relatedness if the investigator is aware of its hazards (B. F. Grimes 1988a:47-51), but it is misleading if assumed to be a valid indicator of linguistic relatedness and closeness.

Modifications attempting to improve the lexicostatistic approach have caused it to be even less valid. One of those modifications has been to change the words elicited in the word list so much that the list is no longer comparable with lists taken elsewhere. Another modification was to eliminate comparison of vowels and only to consider consonants. A modification that affects reliability is in how assumed cognates are counted. Some investigators have counted only words that are identical, others have counted only words they think speakers would recognize, while others have counted only words they think are probably cognates. These three criteria each result in different percentages for the same lists. Such percentages cannot be meaningfully compared with percentages derived elsewhere from different criteria or where the criteria used have not been described.

It is now possible to carry out the comparative method with the aid of a computer (Frantz 1970, Simons 1984, Wimbish 1989, J. Grimes ms.). This means that lexicostatistics no longer has the advantage of being much faster than the comparative method. The latter is more valid than lexicostatistics, and thus more useful in demonstrating language relationships.

Some linguists have compared percentages of proven cognates to arrive at one kind of comparison of lexical relationships. J. Grimes (1964) and F. Agard (1959) used results from the comparative method to compare phonological distance between related language varieties by a method which McKaughan later labeled 'phonostatistics' (1964).

4. Inherent intelligibility among related varieties. A direct test for determining how well speakers of two related language varieties understand each other without additional learning is that described in Casad (1974) and J. Grimes (1974). The method tests intelligibility of recorded text in one language variety by speakers of a related variety. It was initially validated through extensive testing in various language families in Mexico. The results were checked against the impressions of native speakers of the languages and of field linguists who had extensive experience in some of the languages involved. The method has been further validated by testing during a twenty-five year period in many parts of the world and in many different language families.

Intelligibility of narrative, the language sample generally used in this kind of testing, is assumed to give an indication of the intelligibility of other kinds of discourse. If intelligibility of routine narrative is indicated by a mean of 85 perce.. among a population sample, intelligibility of more complex narrative and abstract discourse, and of hortatory, expository, and explanatory discourse will probably be lower. Experience among the Rincon Zapoteco of Mexico (R. Earl, personal communication) and the Campa of Peru (R. Rutter, personal communication) has agreed with this expectation.

Flint (1979, in Fasold 1984.103-105) tested intelligibility of 17 dialogues in Norfolk Island English by a native speaker of Australian English who was a linguist familiar with Creoles, but not with the speech of Norfolk Island. He found that intelligibility percentages on the different dialogues ranged from 32 percent to 80 percent. Although intelligibility increased with later dialogues indicating possible learning, that was not the only variable. Fasold shows that there was a moderate correlation between intelligibility and the percentage of English-related words in a dialogue. (Lexical

similarity with English among the dialogues ranged from 85 percent to 98 percent.) However, these two factors (exposure and lexical similarity) do not account for all the differences in as much as the 15th dialogue (out of 17), with a lexical similarity of 92 percent had only 63 percent intelligibility. Differing grammatical and discourse complexities were most likely also factors in the differing intelligibility scores among the dialogues.

It is tempting to modify valid tests in order to try to get quicker results with less effort. A modified test, however, needs to be validated also because it is no longer the same test. The modification often introduces new variables into the test which may be difficult to measure and the results cannot then be treated as equivalent to those from an unmodified test. A modification may also result in leaving out relevant data.

Some modifications to the recorded text test as described by Casad have introduced unmeasurable variables or omitted important factors. Proposed modifications that affect validity adversely are as follows:

> Testing by group consensus rather than individually results in a population sample of one and obscures the spread in scores, for which the standard deviation is an appropriate measure.
>
> Using the lingua franca for instructions and questions limits the sample to bilinguals and introduces possible confusion or misunderstanding through translation.
>
> Asking the subject for an explanation of the meaning rather than answering a question introduces the individual's ability to rephrase into the results, and if the explanation is done in a second language it introduces his bilingual proficiency and his ability to transfer information into the results.
>
> Using translated rather than natural texts means the language sample may not be valid.
>
> Testing with fewer than ten subjects uses an inadequate population sample.
>
> Omitting important test sites may overlook divergent dialects.
>
> Not screening out those who have had previous contact with the language to be tested biases the population sample.
>
> A modification that decreases reliability is adjusting test scores according to the hometown scores. That compounds possible errors in scoring, and

> leaves in ambiguous or irrelevant questions rather than eliminating them. (Validating the appropriateness of questions on the text by choosing only those questions on which people in the home town score 100 percent, eliminates the necessity of adjusting scores.)

Each of these modifications has resulted in invalid results and means there is a need to go back and test again in a valid way.

Other kinds of tests for inherent intelligibility among language varieties are *indirect* and therefore less valid than recorded text testing (B. F. Grimes 1988a:51-52). They need to be validated or, if they are partial tests, to be used together with other methods.

Some investigators have attempted to use *lexicostatistics* as a means of predicting intelligibility. It is assumed that there is a high correlation between lexical similarity and overall similarity, between lexical similarity and genetic relationship, and between lexical similarity and inherent intelligibility. However, lexicostatistics can be used only as a very indirect approximation of intelligibility inasmuch as it is only an indirect measure for demonstrating linguistic relationships. In addition, it is only a partial measure inasmuch as it does not investigate similarities in grammatical or discourse structure which are important factors in inherent intelligibility (J. Grimes 1988, B. F. Grimes 1988a, 1989).

Agard (1975) has suggested that the *complexity of phonological change* in language divergence may be the basis for dialect versus language distinctions. His investigations have included languages in the Romance and Slavic families. Margaret Milliken (1988) has carried out limited comparisons of such distinctions and intelligibility testing in certain Scottish English and American English dialects, and again with Stuart Milliken in Zhuang dialects (ms.). So far there seems to be a good relationship between the two. This method is an indirect one but looks at a causal factor of which intelligibility is a consequence. It measures complexities of phonological change and needs more replication and correlation with the results of intelligibility testing before it can be used with confidence.

Reading tests for dialect intelligibility actually test reading ability and introduce that variable into comprehension testing. They can be used only with a literate population sample.

Translation tests actually test translation ability and introduce another variable into comprehension testing. People differ in their ability to

translate from one dialect to another, apart from their understanding of the other dialect.

Sentence repetition tests to get at intelligibility between related dialects may actually or partially test mimicry and memory ability (Casad 1974:61), and introduce those additional variables into the results.

Asking the informant which dialects are closest or intelligible involves asking for opinions. Such questions may seem to be direct but they are open to bias from cultural influences, factors in the history of relationships among the speakers, confusion from traditional or ethnic versus language distinctions, or not distinguishing inherent intelligibility from learned bilingualism. It is important to ask for this information but it is not a reliable measure of intelligibility if used by itself.

It is difficult to measure and separate each of the above abilities from intelligibility. Any indirect tests need to be validated by comparing them with the most direct method; recorded text dialect intelligibility testing as described by Casad (1974).

5. Testing bilingual proficiency. The most valid method for testing bilingual proficiency we know about is the *oral proficiency test* developed by the United States Foreign Service Institute (FSI). The procedure has been designed and improved by experienced linguists, psycholinguists, sociolinguists, and language testers. The FSI procedure has been validated by application for over forty years in hundreds of languages around the world, by many testers and linguists, and with thousands of subjects.

The Second Language Oral Proficiency Evaluation (SLOPE) (Summer Institute of Linguistics 1987, B. F. Grimes 1987, 1992) was modified from the FSI test only as much as necessary to enable it to be used in preliterate societies when the linguist may not know the languages being used in the test. It tests as directly as possible what is needed including probing into proficiency and domain limitations (B. F. Grimes 1986, 1988b). It evaluates all five factors that FSI has found to be important in bilingual proficiency: structural precision, lexicalization, discourse competence, comprehension, and fluency. It uses conversational techniques which are natural to the subjects. It does not require literate subjects, testers, or language assistants. Like all adequate bilingualism testing, it needs to be applied to relevant sample groups in each bilingual situation to determine differences in proficiency across the population (B. F. Grimes 1987:8-12).

The SLOPE procedure needs more validating inasmuch as it is a modification of the FSI procedure. The limited modifications were made under the supervision of Thea Bruhn, Head of Testing for FSI. Therefore the results of SLOPE have had some direct comparison with the results of FSI testing.

An indirect test called 'Reported Proficiency Evaluation' (RPE) was used to calibrate the results of sentence repetition testing (Radloff 1987, 1988, 1991). RPE makes use of a modification of the U. S. Peace Corps test, which is a modification of the Educational Testing Service (ETS) test, which is a modification of the FSI test. The ETS modifications and their spinoffs have several problems. They place more emphasis on language knowledge than on communicative competence, and so the focus on grammar is largely on how many grammatical rules the subject remembered or forgot (Radloff 1991:148). They do not treat discourse competence as an important factor. They focus more on lower levels of proficiency rather than the higher levels needed for adequate functioning in all domains. The RPE test has not been validated against the FSI test (p. 128). It uses mother tongue raters, although in the pilot SLOPE study, Thea Bruhn and others found that it was too difficult to train mother tongue testers to do a valid evaluation in the short time, so the SLOPE evaluation is done by the linguist after sufficient detailed checking with the tester and language assistant. The raters in the RPE test are asked to evaluate what they remember of the bilingual proficiency of persons with whom they have been acquainted for several years, thus presenting a serious sampling limitation and also more likelihood of bias (pp. 127, 137). They evaluate their acquaintances on the basis of their own memory of the acquaintances' speaking ability rather than on the basis of observing actual performance in an interview, as with FSI and SLOPE. Subjects are those who communicate regularly with the evaluator in the second language (p. 127). Subjects are ranked with reference to each other (pp. 127-128, 140-141). Educated translators, checkers, and raters are needed because written instructions and other materials are used (p. 127). The raters decide on proficiency first using only the raw scores, and then evaluate the factors related to proficiency thus potentially biasing their decisions (p. 141). All these problems weaken the validity of the RPE test in comparison with the SLOPE test.

Other methods for testing bilingualism are also indirect or partial, and need to be validated and calibrated against the results of the SLOPE procedure. Those other methods include testing comprehension of recorded texts,

questionnaires, observation, translating, sentence repetition, and reading (B. F. Grimes 1987:14-17, 1992).

James, Masland, and Rand (1987) found in Senegal that the results of *recorded text testing* for bilingualism had only a 0.15 correlation with the results of SLOPE testing; too low to establish criterion-related validity for recorded test testing. That kind of testing tests only comprehension, not the more complex factors in bilingualism. It was designed to test inherent intelligibility; it is valid for that purpose but apparently not for bilingualism.

Sentence repetition tests assume that there is a consistent relationship between the ability to repeat something complex and the degree of bilingual proficiency one has. A sentence repetition test is, however, incapable of evaluating a person's ability to initiate structures, his breadth of structural diversity, his lexical discrimination and breadth, his discourse comprehension beyond the sentence, his initiation and production of cohesive discourse, or his use of speech appropriate for diverse situations, at least. However, these are all abilities which are a very important part of higher levels of bilingual proficiency. FSI has found that it is not possible to determine bilingual proficiency by evaluating anything less than all the five factors their test and SLOPE explicitly cover.

In the sentence repetition test (SRT) described by Radloff (1987, 1988, 1991), that test and the 'Reported Proficiency Evaluation' test described above were each used to validate the other (1988:94), even though both are indirect tests, and the RPE test has been weakened by modifications that made it diverge from the FSI test.

A comparative study was undertaken more recently by SIL in Cameroon between the SRT, SLOPE, and the RPE, and reported on by Hatfield, Radloff, Bergman, South, and Wetherill (ms.). The authors conclude (p. 13) that 'The results show a fairly strong relationship between SLOPE and RPE levels, even though there is a wide scatter at some SLOPE levels. The difference is not equal over all levels, but it averages approximately one-half RPE level above SLOPE levels'. Both SLOPE and RPE proficiency levels are defined nominally, not numerically, by definitions which are supposed to be equivalent (Radloff 1991:128), and yet RPE results rated subjects higher than SLOPE.

Wetherill and South (ms.) compared the results of SRT and SLOPE testing from the Cameroon study. They state (pp. 4-5),

> ... the predictive power of the top (flatter) part of the curve is very poor. However, some have argued that the SRT might not be expected to predict higher SLOPE levels well anyway, so that, we could cut off the top part of the curve as invalid. On this basis we could fit a straight line to the bottom part of the curve, which would have a very limited range of validity.

The main report concludes (p. 21),

> With reference to establishing a cut-off point, the results show that the French SRT A would give good results if the cut-off point were SLOPE level 3+ (levels 0+ to 3 versus 3+ and above), but that we could not be very confident in our results for a SLOPE level 3 cut-off point.

Varenkamp and Varenkamp (ms.) point out that the SRT in South Asia showed only how bilingual a community is *not*, and was not used to show how bilingual a community *is*.

The assumption is made that once the results of an SRT in a given second language by speakers of a given first language are calibrated against the results of an RPE test, the same calibration can be applied to any second language speakers of that language regardless of their first language (Radloff 1991:29). This is based on an assumption by some language teachers that second language learners of a given language make the same progress and mistakes regardless of their first language. That assumption is not true with respect to phonology and has not yet been demonstrated to be true with respect to grammar or discourse structure. There needs to be more replication with a single second language and various mother tongues before it can be concluded that an SRT can be used with the same calibration among speakers of various mother tongues.

Training and preparation appear to take as much, if not more, time for this SRT as they do for the SLOPE test. Administering the SRT on the field takes less time than SLOPE.

Questionnaires have been found to be unreliable, especially as the only indicator of bilingual proficiency. Bias can easily enter into the subject's responses and the investigator's evaluation (Fasold 1984:147). Quakenbush (B. F. Grimes 1987:16) and FSI both found self-report checklists to be unreliable when compared with the results of oral proficiency interviews. Questionnaires can be useful for gathering information, however, if used together with a valid test.

Observation depends on the training, experience, and sensitivity of the evaluator. As a tool for evaluating bilingual proficiency, the evaluator needs to be highly proficient in the language being evaluated.

Translating and reading tests to determine bilingual proficiency introduce new unmeasurable variables into the situation that are difficult to distinguish from bilingual proficiency. They are separate abilities. When bilingualism in preliterate populations is being evaluated, reading tests can be given to only a small proportion of the population. That proportion is likely to be the most bilingual group and the investigator is likely to get an invalid picture of the entire society. FSI has found no regular correlation between reading ability and bilingual proficiency.

Since studies to date show that an SRT may be useful as a screening test for lower levels of bilingual proficiency up to SLOPE/FSI level 3, but cannot distinguish level 3+ from those above, SLOPE testing is the most valid method we have for evaluating higher levels of bilingual proficiency. SLOPE would also be more valid for calibrating SRT's than RPE.

The desire for quicker testing procedures should not be allowed to override the importance of validity and accuracy in survey results.

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SIXTH INTERNATIONAL SYSTEMIC-FUNCTIONAL WORKSHOP
Spoken And Written Discourse in Institutional Contexts
8 - 11 August 1994
Sponsored by The Centre for Applied Linguistics (ICTL)
of the University of Antwerp (UFSIA).

Deadline for submitting abstracts (31 March 1994) was past before scheduled publication of this issue of *Notes on Linguistics*. Registration fee for the workshop will be approx. $75 or £50) including lunches on 9, 10 and 11 August. A choice of simple student hostel accommodation ($20) per night include breakfast) will be available within walking distance of the workshop, which will be held at the UFSIA campus. For further information write:

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Recent developments in CECIL

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I have recently heard various interesting comments about CECIL, an acoustic phonetics software and hardware package developed in the Summer Institute of Linguistics.[1] It seems that some SIL Branches and members are using it extensively while others are scarcely using it. Of course, those working on tonal languages may be able to make the most productive use of CECIL, but it would be a mistake to think that CECIL is only beneficial for people working on tonal languages.

Papua New Guinea (PNG) and Irian Jaya, part of Indonesia, are two halves of a large island with an excess of 1000 languages there. I am told that in PNG CECIL is scarcely used, but in Irian Jaya one team has probably used CECIL more extensively than it has been used for any other single language.

I was therefore interested to meet two members from PNG who are working on a non-tonal language. They have had some problems with the phonology of the language they were investigating, so they decided that while spending some time in the USA they would take a university course in linguistics to get the skills they needed to solve those problems. It was at the university that they were advised to obtain the SIL CECIL package to help them solve their problems. I wonder how many other people in SIL need this sort of help and are not getting it.

The main point of this article is to inform readers of the recent developments involving CECIL. In a recent edition of *Notes on Computing*, the availability of CECIL version 2.0 was announced. This version has some significant advantages over previous versions, but there are also some problems in changing from a previous version to this one.

The problems are:

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[1] CECIL is available from JAARS, Intl. Computer Services, P. O. Box 248, Waxhaw, NC 28173-0248. (Internet: icscust@a1.jaars.sil.org, FAX (704) 843-6200, Phone (704) 843-6085.)

1. A new menu system has to be learned. (It has been changed to be more compatible with other programs.)
2. Documentation is only bridge documentation, noting only the differences between version 2.0 and version 1.2.
3. For those using a foreign language version of CECIL, a substantial amount of retranslation will be necessary.

The advantages are:

1. Support for VGA screens and better use of DSCGA screens.
2. The inclusion of the SPECTRUM program.
3. Easy measurement of the duration of segments for utterances that have had the phonetic transcription accurately marked.
4. Naming of files that are to be saved to disk, e.g. PCX graphics files.
5. Much better display of spectrograms on screen and in graphics files.
6. Improved spectrogram algorithm.
7. Provision of a second cursor.
8. Ability to define the duration over which a Fourier spectrum should be calculated.
9. Shows spectra changing with time (Video spectra).
10. Learning the new menu system will better prepare the user for new versions.
11. Provision of two utility programs; SLOWUTT for the better slowing down of utterances, and REPLAY for helping with language learning.

Version 2.0 is available now, but I am presently working with a beta version of CECIL 2.1. The program has no known bugs, but we have further testing to do. The advantages of version 2.1 over version 2.0 are:

1. Substantially improved slowed replay, so that SLOWUTT is no longer needed.
2. REPLAY has been replaced by the more friendly UTTPLAY for language learning, also providing the much improved slowed replay.
3. An additional program WARPUTT, which allows the changing of duration and/or loudness (amplitude) of specific parts of an utterance. This has been requested by some phoneticians to permit testing of perceptual reactions to such changes. The program, though, has at least two other uses: modifying an utterance that was recorded too quietly and isolating particular sounds from a group of utterances.
4. Changing the default sampling rate to 22,000 Hz. Although this may be regarded as a disadvantage, since it reduces the total time of an utterance from 3.3 seconds to just under 3 seconds, it prepares the way for having data that can be easily and accurately transferred to

Windows and Macintosh audio formats. However, the user can still specify 19,500 Hz as the default sampling frequency, and earlier recordings will still play at their correct speed.

Manuals are being prepared for CECIL 2.1. Ed Owen is working on the new Reference Manual. I am currently revising the CECIL linguistics manual (Interpreting CECIL: Gathering and Interpreting Acoustic Phonetic Data).

Cecil 2.1 is likely to be the final MS-DOS version of CECIL. Philip Brassett, who has programmed all the versions of CECIL until now, is presently working on a Windows version of CECIL, and we are hoping to get this Windows version ported across to Macintosh computers. The Windows version will require different hardware from the present CECIL boxes, but it is hoped that such hardware will gradually come built-in to even portable computers. Macintosh computers already come with such a built-in device.

SIL involvement with acoustic phonetics does not stop at this point. Terry Gibbs and Alec Epting are working with Philip Brassett and me to extend the power of what is presently available. Hopefully, the next generation of acoustic phonetics tools will do all that CECIL does and a lot more, but there is much research that must be successful before this can happen. With CECIL it was a case of doing what we knew was possible; from now on we cannot be sure that everything we hope for is possible.

In closing I want to comment on the use of CECIL in language learning, and I do this because of a negative report I received from one SIL entity where CECIL is used extensively. It seems to me that there are two ways that CECIL can be used in language learning:

1. It can be used to train the user to recognize and reproduce the general characteristics of the language being learned. This might include hearing and reproducing tone properly or identifying particularly unusual sounds. In this mode CECIL should probably only be used for a couple of months, and the learner should then be able to rely on his ear thereafter.

2. CECIL together with the UTTPLAY program can be used in place of tape loops. About 150 drills of six utterances each can be stored in about 20 Mbytes of a hard disk, giving fast access in any other, including random practice and slowed speech. This could serve for ongoing use.

But real language learning is done by listening to people and talking to people. If CECIL becomes a substitute for that, then it is doing the learner a disservice. ■

FOURTH INTERNATIONAL SYMPOSIUM ON LANGUAGE & LINGUISTICS
Central Plaza Hotel, Bangkok, Thailand - January 8-10, 1996
Institute of Language and Culture for Rural Development
Mahidol University, Thailand

This Symposium is organized once every four years in Thailand. The last symposium hosted by Chulalongkorn University, Bangkok 1992 had as its theme **PANASIATIC LINGUISTICS**. The same theme is to continue at this symposium and will bring together scholars interested in linguistic studies of Asian languages from Japan to the Middle East, and from Siberia to Indonesia. All are welcome. Papers on all aspects of language description are invited.

Registration Fees: Before Aug. 31, 1995 $130; After Aug. 31, 1995 $150.
Payment by draft made to PanAsiatic Linguistics Symposium.

Deadlines: Aug. 31, 1994 - Submission of preliminary registration form
Jan. 31, 1995 - Submission of abstract (in English)
Aug. 31, 1995 - Submission of paper

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On the motivation for feature geometry*

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SIL—Peru Branch and North Dakota SIL

In the past ten years a branch of phonology known as Feature Geometry has entered linguistic theory, drastically altering our conception of the internal structure and composition of speech segments. The purpose of this paper is to summarize for the uninitiated reader the lines of argumentation which have been proposed in order to justify this revision in linguistic theory. That is, whenever phonologists seem to be getting along fine with 'the good old way' of doing things, the burden of proof must be borne by those who claim that the current formal mechanisms are inadequate and that therefore the theory needs to be revised or expanded.

Feature Geometry, the recent proposal in focus here, is the arrangement of distinctive features into a hierarchically structured tree such as on the following page. This particular model of feature geometry has been used since 1989 in the introductory phonology course taught at North Dakota SIL, and has also been introduced in some other SILs. It is not my purpose to argue here for one particular feature tree among the many which have been proposed (e.g., Clements 1985, Sagey 1986, and McCarthy 1988). Rather, I would like to defend the decision to teach from this perspective in introductory phonology courses. In the following pages I will discuss five reasons why most phonologists have embraced feature geometry as an improvement in the formal representation of phonological structure and rules.

1. It has been claimed that the mechanism of autosegmental[1] spreading describes assimilation as a sharing of the same articulatory gesture more directly and explicitly than does the changing of a feature value in a matrix

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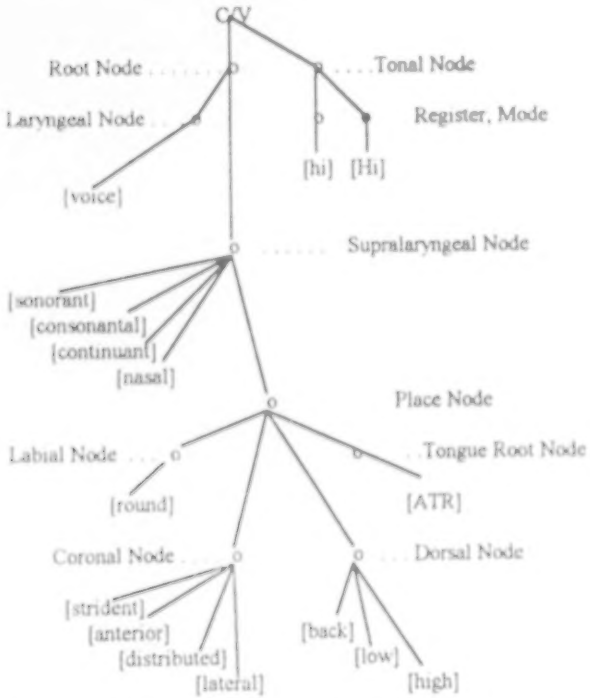
* Thanks to Steve Marlett for encouraging me to write this paper, and to Andy Ralph Eatough and Jim Meyer for reviewing it.

[1] The term 'autosegmental' here refers to a formal representation in which certain distinctive features are placed on their own nonlinear tier separate from but parallel to other feature tiers (Goldsmith 1976).

(1)



(Sagey 1986:23). Thus, feature geometry is physiologically and descriptively more satisfying as a formal representation of what happens when we speak. For example, consider a process in which obstruents acquire voicing from a following voiced consonant. In terms of the classical model of generative phonology based on *The Sound Pattern of English* (Chomsky and Halle 1968; hereafter SPE), this process can be formalized as follows:

(2) $C \rightarrow [+voice] \; / \; \underline{\quad} \; \begin{matrix} C \\ [+voice] \end{matrix}$

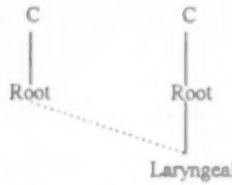
This notation implies that we have adjacent underlying matrices which contain the specifications:

(3) [-voice] [+voice]

and that as a result of applying the rule in (2) to this string of segments, the value for the feature [voice] in the first matrix is changed from '-' to '+'.

On the other hand, in a feature geometry approach, this rule would be formalized as a spreading of the Laryngeal Node:

(4)



This representation is inherently superior to that of (2) since after the application of the rule in (4), the resulting structure will contain a single Laryngeal Node simultaneously linked with two linearly adjacent segments. This view of assimilation explains the phonetic motivation for the voicing—anticipatory vibration of the vocal cords—in a way which is much more graphically and intuitively correct.

2. In this approach it is easy for the linguist to formalize simple, natural processes and more difficult to express complicated, unnatural processes. This criterion is also true to a certain extent for the classical SPE notation, but to a lesser degree. This point is closely related to the previous argument (assimilation = spreading) in that in formalizing rules using feature geometry, it is impossible to spread a feature or node which is not already present somewhere in the triggering environment. For example, consider a rule such as the following:

(5) [+aspirated] → [-consonantal] / —— C
[+distributed]

This bizarre rule, which would change /p^h/ into something like [W] before a segment such as /š/, describes a process which is not known to occur in any language of the world and should therefore be considered highly marked and phonologically implausible. Nevertheless it is just as easy and simple to express this rule (in SPE terms) as it is to write rule (2).

According to the evaluation metric of classical generative phonology, the simplicity and naturalness of a particular rule can be determined by counting the number of features used to formalize it and comparing that with the total number of features used in a competing analysis. Following this logic we would be forced to conclude that rules (2) and (5) are equally simple and natural, which, of course, is a false evaluation. In a feature geometry approach, on the other hand, the formalization of the process referred to in (5) would be inherently more complex and ad hoc than the

process described in (2) and (4). This is a desirable consequence since it more correctly reflects phonetic reality. This is the second advantage in favor of the feature geometry model.

3. The arrangement of the feature tree into hierarchical class nodes allows us to more simply express phonological processes which involve the simultaneous assimilation of more than one distinctive feature. For example, consider a nasal assimilation process in a language which contrasts four points of articulation for consonants: bilabial, alveolar, alveopalatal, and velar. To distinguish these four places with SPE conventions, we need to rely on an interaction between two variable features:

(6) $$\begin{matrix} C \\ [+\text{nasal}] \end{matrix} \longrightarrow \begin{bmatrix} \alpha \text{ coronal} \\ \beta \text{ anterior} \end{bmatrix} \; / \; \underline{\quad} \; \begin{bmatrix} C \\ \alpha \text{ coronal} \\ \beta \text{ anterior} \end{bmatrix}$$

Sadly, this rule is already more complex in terms of the quantity of features referred to than the voicing assimilation rule formalized in (2) above. What is more, the situation becomes even more complicated if we compare rule (6) with an analogous nasal assimilation process in some other language which systematically exploits more than four consonantal points of articulation, such as Hindi. In this case, in order to capture the contrast between dental, alveolar, and retroflexed coronals, we are forced to add to our rule a third distinctive feature, such as [±distributed]:

(7) $$\begin{matrix} C \\ [+\text{nasal}] \end{matrix} \longrightarrow \begin{bmatrix} \alpha \text{ coronal} \\ \beta \text{ anterior} \\ \gamma \text{ distributed} \end{bmatrix} \; / \; \underline{\quad} \; \begin{bmatrix} C \\ \alpha \text{ coronal} \\ \beta \text{ anterior} \\ \gamma \text{ distributed} \end{bmatrix}$$

This is an unfortunate situation since SPE theory would have to conclude that the nasal assimilation process in Hindi is more complex, more marked, and hence less natural than an identical process in a language which has only a four-way place distinction among consonants.[2]

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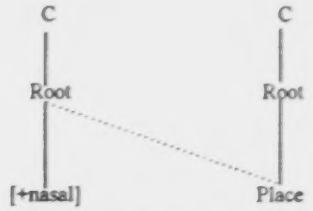
[2] I note that the feature [α place], which was commonly used as a convenient abbreviatory device throughout the literature of the period, had no formal status in the theory and hence should not be considered any less cumbersome than rules (6) and (7). The very fact that it was even invoked at all points to a deficiency in SPE theory.

In the feature geometry approach this dilemma is resolved very nicely by simply spreading the Place Node in both cases:

(8)



Since the Place Node dominates all and only those features which define the distinct points of articulation, spreading it will suffice to make the nasal homorganic to any consonant which follows it, regardless of how many places are distinguished by the language in question. Thus this notation correctly reflects the fact that the nasal assimilation processes (6) and (7) are equally simple and natural. Furthermore it also implies that this process is just as unmarked and expected as the voicing assimilation process formalized in (4).

This brings us to a very important point to note with respect to current phonological thinking: One of the most basic and fundamental insights of the feature geometry system is that every phonological process which is truly assimilatory in nature should be able to be formally represented by the addition of a single association line somewhere in the structural description of the rule. Therefore the model of feature geometry which we employ must be designed in such a way that any group of two or more features which are jointly assimilated as a unitary process must be uniquely and exhaustively dominated by a common class node. This pressure has led to many revisions and modifications of the feature tree, and phonologists are still not in total agreement about which features and nodes (as well as their hierarchical arrangement) will ultimately lead to the ideal formal configuration. Nevertheless, on the strength of arguments such as those outlined here, linguistic theory has made some very positive advances in the last decade.

4. This is more constrained, more restricted, and less powerful than the SPE notation. It is a desirable benefit since two of the goals of Universal Grammar are (1) to avoid unnatural abuses of our formal devices and (2) to limit the number of possible grammars among which the language learner potentially has to choose.

One example of the unrestrained power of SPE theory is that it permits us to group together the class of sounds /p b k g/ to the exclusion of /t d č ǰ/, by means of the feature [-coronal]. It is not clear that this is a move which linguistic theory should allow since it is doubtful that labial and dorsal consonants truly pattern as a phonological natural class in any language of the world.[3] Thus in most models of feature geometry, the tree is set up in such a way that [coronal] functions as a class node and is therefore by definition a privative (monovalent), rather than binary, feature. Consequently an automatic benefit of the theory is that now it directly rules out an unnatural use of the feature [-coronal] since there is no way to formally refer to the absence of a node.

Similarly, many models of feature geometry restrict the power of features such as [anterior], [lateral], [distributed], and/or [strident] by subsuming them under the Coronal articulator node. In a theory of features which is not constrained along these lines, one could group together the class of segments /f s š/ by means of the feature [+strident]. The drawback of this mechanism is similar to the problem discussed above regarding [±coronal]: No language in the world is known to exhibit a process requiring us to refer to /f s š/ as a group to the exclusion of other voiceless fricatives such as /ɸ θ x/, which were traditionally considered to be [-strident]. Thus by limiting features such as [strident] to a particular articulator node, as many models of feature geometry do, we are able to severely curtail abuses of this sort.

5. Another advantage of the feature geometry approach to the organization of distinctive features is that it more explicitly and directly predicts, in the very formalism itself, what types of segments can and cannot exist in the languages of the world. One of the objectives of linguistic theory is to explain why those segments which do exist, can exist, and why those segments which do not exist, cannot, and the various feature geometry trees achieve this goal much better than SPE formalisms do. For example, imagine a hypothetical segment in some language X defined by the combination of features in (9):

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[3] I am assuming that the tendency of labial and dorsal consonants, but not coronals, to block processes of spreading can be handled by appealing to underspecification theory (cf. Paradis and Prunet 1989), rather than by modifying the feature geometry tree to permit an equipollent (binary) use of the feature [coronal].

(9) $\begin{bmatrix} \text{+round} \\ \text{- labial} \end{bmatrix}$

This logical paradox is possible in classical generative notation since in that theory there is no formal way to a priori rule out such absurdities. Now of course no linguist in his right mind would ever come up with such a combination of features, but my point is that there is no stipulation within the matrix system per se which tells us that the features [+round] and [-labial] are less compatible than any other arbitrary pair of features such as, for example, [+round] and [-high]. This is another instantiation of the fact that SPE notation is too powerful in that it can generate problems of this sort, forcing us to patch things up later with redundancy rules and other filtering devices. The feature geometry mechanism automatically avoids dilemmas such as (9) since most trees are designed so that the only possible way to access the feature [round] is by means of invoking the presence of the Labial Node. There is no provision in current models for formally referring to a feature such as [-labial].

In summary, I have presented five primary lines of argumentation which show that the feature geometry approach to the internal representation of segments is empirically superior to the corresponding mechanisms of SPE vintage.[4] For reasons such as these, most phonologists in the generative tradition have abandoned the classical notation in favor of some model of hierarchical, autosegmentalized trees. In the interest of producing linguistic analyses and descriptions which are useful to the academic world at large, it behooves SIL to utilize the recognized advances in formal linguistic theory.

Reflections

I can think of no better way to achieve this goal than to incorporate enrichments such as feature geometry into linguistic training programs from the very first course. This emphasis better prepares students to

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[4] As an aside, one additional advantage of feature geometry theory is that by formalizing assimilation processes via autosegmental spreading, it is concomitantly easier to avoid violations of the Obligatory Contour Principle (OCP). This argument is perhaps less compelling than the other five since it is more dependent on theory-internal presuppositions. The OCP stipulates that at the melodic level, adjacent identical elements are prohibited (McCarthy 1986; Odden 1986; Yip 1988). Classical generative notation, which conceives of assimilation as the changing of a feature value in a matrix, regularly leads to OCP violations. Although this is admittedly a controversial subject, most phonologists would agree that all other things being equal, it is best to conform to the OCP whenever possible.

interact with linguistic literature, which is becoming increasingly technical. Even in journals like *IJAL*, for example, which have traditionally leaned more heavily toward a data-orientation than a theory-orientation, recent articles assume a basic working knowledge of the feature geometry approach (cf. Doak 1992).

I do not mean to imply by these statements that all the complexities of feature geometry theory should be taught in the first phonology course. It is appropriate at that level to use a 'scaled-down' model such as (1) and focus on the basic concepts. My experience has been that beginning students quite readily identify with this approach since it utilizes intuitive groupings of feature bundles such as Place, Laryngeal, etc. Furthermore, it helps them write more natural rules by constraining their choice of formalisms from the outset. Finally, when handled in an appropriate way, the feature tree is no more difficult to teach than are other models.

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REVIEWS

The Cognitive Paradigm: An integrated understanding of scientific development. By Marc de Mey. Chicago: The University of Chicago Press. 1992. 314 pp. Paper $15.95.

Reviewed by Graeme Costin
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In *The Cognitive Paradigm* Marc de Mey is attempting to do more than merely understand the psychological and social makeup of scientists. He is attempting to move forward to a scientific theory of how people learn and know. This is an area of great interest to people studying artificial intelligence who would like such a theory in order to produce better computer software for artificial intelligence applications. One of the reasons why the introduction to *The Cognitive Paradigm* may be so difficult for the uninitiated to read is that it is largely a lament concerning the lack of progress made by cognitive science during the decade since first publication of the book. In fact, it discusses issues in a way that assumes the reader has already read the book.

I decided to attack this intellectual edifice by homing in on something with which I was familiar. I had recently read the part of *Inside Macintosh Volume VI* which describes the gestalt functions which allow a Macintosh application program to find out the precise variant of system software in use so that appropriate action can be taken to use the capabilities present or to take alternative action. The word **gestalt** is not in the average English speaker's normal vocabulary—why did Apple Computer Inc. use it?

Marc de Mey uses gestalt often; the index of his book has 12 references to it. The first reference (on page 90) mentions the **gestalt switch** which shows itself in a 'change of paradigm':

> After a change of paradigms 'scientists see new and different things when looking with familiar instruments in places they have looked before'.

This is easiest to see in the ambiguous figures studied in Gestalt Psychology such as the pelican-antelope figure.

Marc de Mey says that 'perception in science is like seeing the pelican in one paradigmatic episode and seeing the antelope in another paradigmatic episode. The gestalt switch shows that the world can look very different even when the data remain the same'.

But what is meant in this context by **paradigm**? Marc de Mey presents a dissection of this type of paradigm from work by T. S. Kuhn (who wrote fourteen of the items listed in de Mey's Bibliography), as well as from work by Lakatos and by Musgrave, as follows:

symbolic generalizations	Formulas or verbal statements which express fundamental laws or principles. Example: $f = ma$ in Newtonian mechanics.
metaphysical beliefs	Preferred analogies combined with an ontology. Example: A gas is a collection of billiard balls in random motion.
values	Methodological requirements with which scientists try to comply. Examples: simplicity, consistency, accuracy, testability.
exemplars	Concrete embodiments of the symbolic generalizations and metaphysical beliefs. Example: A novice in science studies the pendulum as a solitary case, and later can see it as akin to free fall, satellite orbiting, etc. as the symbolic generalizations and metaphysical beliefs are built in his mind.

One of the many points brought out in Marc de Mey's book is that the psychology of scientists is a significant element in science; the first two parts in the above dissection of a scientific paradigm are not just incidental—they profoundly affect the way the scientist perceives the facts obtained by scientific experiment. They can either assist or hinder further scientific progress. A linguist's commitment to the metaphysical beliefs of

stratificational grammar (or government and binding grammar, or tagmemic grammar, or role and reference grammar, etc.) may be strong enough to prevent him perceiving how some other grammar theory might better fit the language he is studying. An anthropologist's commitment to metaphysical beliefs about the sanctity of life may prevent him understanding how a Hindu views infanticide of female babies. If we understand better how our psychology interacts with the way we do science, we may be able to do better science.

Those who have graduated from a university course in cognitive science should have no difficulty reading this book from cover to cover (they may already have done so during their course). The rest of us may find that very difficult. The uninitiated reader might start by looking up something familiar in the index. Those references could show how that particular idea relates to other aspects of cognitive science and could eventually lead to an appreciation of the whole book. But the uninitiated reader should be prepared to learn new meanings for some familiar words! In my case I found that **gestalt** as used in *Inside Macintosh* is closely derived from **gestalt** as used in Marc de Mey's book, but it is not identical. ■

21st INTERNATIONAL SYSTEMIC FUNCTIONAL CONGRESS
University of Gent, Belgium 1-5 August 1994
- Conference Theme -
FUNCTIONS OF LANGUAGE

Keynote Speakers
Basil Bernstein (London), Claude Hagège (Paris), M.A.K. Halliday (Sydney), Ruqaiya Hasan (Macquarie), Christian Matthiessen (Sydney), William McGregor (Melbourne), Petr Sgall (Prague), Anna Siewierska (Amsterdam), Stanley Starosta (Hawaii), Jef Verschueren (Antwerp)

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Copy-editing: The Cambridge handbook for editors, authors, and indexers. 1992. By Judith Butcher. 3rd ed. Cambridge: Cambridge University Press. Pp. 384. Hardback $44.95.

Reviewed by Genevieve M. Hibbs
Wycliffe Associates—UK

1. Although I am not a copy editor, this book offers much that is relevant to my international, information-handling research and teaching interests that is not easily available or in one place elsewhere. Any who prepare publications could find it helpful. I highly recommend it.

This third edition of Butcher's book is rewritten and expanded to include sections on typescripts produced by the author on disk, unbiased and nonsexist writing, and special calendars. Although prescriptive in places, it usually describes a range of variations often pointing out the preferred one. Prescription is reassuring in such a complex situation.

For me and many others, all 13 appendices are likely to be useful as a first point of call. The checklist of copy editing, in particular, looks very practical, and the Islamic and other calendar data are potentially very useful, but need to be cross referenced back to some very practical comments in '6.5 Dates and time', especially from the last paragraph of page 135.

Being unfamiliar with several of these comments, I found the explanation of date abbreviations (p. 135): AC AD AH BC BP CE ad bc bp, and how they are used, to be rather confusing, and I wondered whether a chart with their respective interpretation and rules might help to clarify the situation.

Attempt has been made to take full account of new technology, something which was difficult to encompass during the past ten years when so much was in flux and direction was often difficult to determine.

2. Comments on specific topics.

2.1 Bibliographic entries. Open punctuation, by omitting redundant points and commas could be commended. It gives a cleaner appearance in any case.

I find it hard to understand the rationale for using capital letters for other than proper names and the first word in book titles. Tradition seems an insufficient justification when the process leads to loss of information and confusion. The problem is more obvious when dealing with foreign

language titles when one is not already familiar with which words are proper names, and therefore should or should not be capitalized in normal text. For very many readers English titles are foreign language titles. Even many native English speakers are totally confused about capitalization, a problem which this practice helps to perpetuate. Additionally, when handling text on computer, consistency of capitalization of a word throughout a text is advantageous for many routine text manipulations for business and academic purposes.

2.2 Contents list. In reviewing another book I recently suggested that there was sometimes a case for putting missing sections in the contents list and pointing to where the matters are which traditionally are dealt with there, as well as an explanatory note where appropriate. For example, where there are technical words used but they are defined adequately within the text, 'glossary' is listed in the contents list as not existing, but with a cross reference to 'index' and a note to say that definitions within the text are indicated by page numbers in italics. I would like to see this principle of marking missing sections, where necessary, included in a future edition.

2.3 Alphabetization order. It would be helpful to have order of alphabetization indexed and defined more precisely. There are choices which must be made about order where there are hyphens, apostrophes, and quotation marks, as well as the foreign characters mentioned elsewhere. The implications of computer ASCII order sorting is also appropriate to mention in this respect.

2.4 Index. I would like to see glossary entries in this text consistently indexed. In any case many of the words are already indexed for other purposes, which may include definition but even then, inconsistently mentioning the glossary page number. The nature of the likely use of this reference work requires it. ■

The legacy of language: A tribute to Charlton Laird.
Edited by Phillip C. Boardman. Reno: University of Nevada Press. 1987.
187 pp. Hardcover $24.95

Reviewed by James A. Lander
SIL—East Africa Group and Texas SIL

This work honors the memory of Charlton Laird. He was so well thought of by his peers that they sought to hold a symposium in his honor. Sadly, Laird died before the conference convened. Laird's admirers and friends changed the symposium to a memorial and met as originally scheduled in November 1984. Twelve papers presented at the memorial were published in this volume.

The papers, however, do not merely sing the praises of Laird. As Phillip Boardman wrote in his introduction, the book seeks to provide '...the ordinary reader new insight in the workings of language and the discoveries of linguistic scholarship' (xv). It does that well, particularly in explaining some of the ways society and language interact with each other.

Naturally, not all of the papers appealed equally to this reviewer. Nor would the articles which charmed him necessarily absorb other readers to the same degree. Many of those who are interested in the way language works should be satisfied with this book. However, those who favor prescriptive linguistics as opposed to descriptive linguistics will find little comfort in the text.

Harry Brent's article, 'And gladly teche: "Stedfastnesse" in the Clerk's tale and in the pedagogy of Charlton Laird', praised Laird more profusely than the others, though no more highly. This reviewer found little of interest in Brent's contribution. That is due to the reviewer's disinterest in Chaucerian English, not the worth of the piece. Brent offered an alternative interpretation of the Clerk. Those interested in the Canterbury Tales will be interested in Brent's offering.

Lexicographers might take interest in William H Jacobson, Jr.'s 'The root of the matter: Reflections on English etymological dictionaries', and in 'The lexicographer as social chronicler', by David B. Guralnik.

Robert H. Bentley, in 'Social dialects: Educational implications of the study of Black English', aptly called for the results of linguistic research to be made available to the common soul. His reason? According to Bentley, hostility toward nonstandard dialects decreases after instruction in

linguistics. In other words, the denigration inflicted on members of minority groups due to their use of nonstandard English decreases when the perpetrators understand the nature of language.

In James Sledd's article, 'Language and social class', the author demonstrated how prescriptive grammarians use language to separate themselves from those they perceive to be of a lower class. That is, he asserted, '...standard English has always been...the English of the dominant and an instrument of their domination' (p. 98). He, perhaps surprisingly, did not advocate the abandonment of standard English. He suggested instead that standard English be functional and not arbitrary. It should be available to all, not just the elite. He further stated that whenever one is unable to master Standard English, that fact should not be used to deny that person economic opportunity or social acceptance.

In 'Language, appearance, and reality: Doublespeak in 1984', William D. Lutz examined the deliberate use of language to deceive the common American. He contended doublespeak corrupts thought and associates it with Orwellian oppression. That premise reveals some of the writer's understanding of how language and society interact with each other. Lutz urged teachers of English to instill their students with an affection and respect for language which would cause them to respond to doublespeak with outrage. His sentiment is one with which this reviewer easily identifies.

What makes good English good? John Algeo offered his answer in a paper using that question for its title. Algeo probed standards offered by a well known prescriptive grammarian, Theodore M. Bernstein, among others. Algeo rejected them all, concluding that good English is language appropriate to the social circumstances in which it occurs.

In 'Misunderstanding standards of usage', Thomas L. Clark expanded on Algeo's theme. Appropriate language, he explains, is not synonymous with correct language, nor antithetical to incorrect language. Appropriate means, 'proper to the time and situation' (p. 133). Thomas did not argue that spelling and usage should not be taught; rather they should be included along with the role of language in thought and society.

Edward Finegan's, 'On the linguistic forms of prestige: Snobs and slobs using English', explored some of the ways differing strata of society use language. He made the case that differences in usage between socioeconomic classes is systematic rather than arbitrary.

Walker Gibson examined language change in his article, 'Usage and style: 1984 and 1066'. Walker noted that grammarians often resist change in language usage and forms. He concludes that it is useless to resist language change and urges readers to maintain an open mind to new language forms and uses.

The legacy of language: A tribute to Charlton Laird includes a useful listing of selected publications of Charlton Laird which was compiled by Anne K. Phillips. Also included is a seven page bibliography and a five page index. The index covers persons and subjects.

This book is a fitting tribute to Charlton Laird. It also contributes to the study of some of the ways society and language interact. The articles are thoughtful as well as thought-provoking. For admirers of Laird, this book is a 'must-read'. Language lovers who may not be familiar with Laird should read it, too. ■

Phonologica 1988. By Wolfgang U. Dressler, Hans C. Luschützky, Oskar E. Pfeiffer, and John R. Rennison. Cambridge: Cambridge University Press. 1992. 298 pp. $64.95

Mike Maxwell
SIL—International Programs

This book consists of the majority of the papers given at the Sixth International Phonology Meeting in 1988, as later revised by the authors. Like the conference proceedings of the Chicago or Berkeley Linguistics Societies, the papers are quite short (about ten pages each). Such a short paper is sufficient for developing a very specific topic; unfortunately, many of the authors in this volume appear to have chosen more general topics. The resulting papers are often too short to develop the chosen themes, instead remaining programmatic or serving largely as bibliographic references to the authors' work developed at length elsewhere.

An example is Allan R. James' 'Prosodic structure and lexical representation: the role of features'. James claims that in the phonological hierarchy, the head of a constituent must be defined as both the most prominent constituent (e.g. a stressed syllable) and the most contrastive

constituent.[1] But argument for this position is lacking; he instead refers to a lengthier work of his published elsewhere.

Likewise, the complex argumentation in Angeliki Malikouti-Drachman and Gaberell Drachman's article 'Greek clitics and Lexical Phonology' is largely impossible to follow without reference to the articles they criticize. A journal-length article would have summarized the arguments of the authors Drachman and Drachman argue against, but there was no room for such a summary in the short space allotted them in this volume.

A few papers are longer, and in general these are more successful in presenting their case. For reasons of space, I will discuss in the following subsections only those papers which I deem likely to be of greater interest to the readers of *Notes on Linguistics*. The authors and titles of the remaining papers are: Kristján Árnason, 'Problems in the Lexical Phonology of Icelandic'; Robert Bannert, 'Tonal elements and tonal features in sentence level intonation'; Hans Basboll, 'Stod in present-day Standard Danish: A case of phonology-morphology interface'; Pier Marco Bertinetto, 'The use and misuse of external evidence in phonology'; Anne Cutler, 'Why not abolish psycholinguistics?'; Carlos Gussenhoven, 'Intonational phrasing and the prosodic hierarchy'; Henry Hock, 'Initial strengthening'; David House, 'Perceptual constraints and tonal features'; Allan R. James, 'Prosodic structure and lexical representation: The role of features'; Haruo Kubozono, 'On the metrical structure of Japanese downstep'; Peter Ladefoged, 'The many interfaces between phonetics and phonology'; David Michaels, 'Natural and unnatural phonology'; Maria-Josep Solé, 'Experimental phonology: the case of rhotacism'; Rebecca Treiman, 'Experimental studies of English syllabification'; and Sidney A J. Wood, 'A radiographic and model study of the tense vs. lax contrast in vowels'.

Geert Booij, 'Lexical Phonology and Prosodic Phonology'

Working in the theory of Lexical Phonology, Booij argues that prosodic structure must be built at the lexical level, and may be changed after the application of each cyclic phonological rule. (Booij actually discusses only syllable structure, remaining silent about other prosodic levels.) As Booij recognizes, resyllabification complicates derivations. Accordingly, he

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[1] Allan confusingly uses the term 'constituent' to mean 'head'; I use this latter term here.

proposes a constraint: only syllable codas may be erased from existing syllabic structure.

Booij's proposal is an interesting one but it remains programmatic. The paper is simply too short to firmly establish either the need for resyllabification or Booij's particular proposal of how resyllabification takes place. At several points, alternative analyses may occur to the reader. An example is the discussion on pages 59-60 of resyllabification after epenthesis. The structure shown in Booij's example (19) may indeed be ill-formed (although no criterion for deciding this is given), but one wonders whether the well-formedness check on syllable structure could be postponed until resyllabification is complete. Perhaps not, but the brevity of the article leaves this unclear.

The choice of evidence could also have been better at times, as for instance when Booij uses Spanish as an example of a language with cyclic stress (citing Harris 1969). While a number of authors have followed Harris' analysis of Spanish stress as cyclic, the evidence for this claim is, in my opinion, very slim, being based on questionable synchronic derivations.

The question of resyllabification is a difficult one; Booij's approach is interesting (and presents a partial counterpoint to Ito 1986, which Booij unfortunately does not cite), but needs further development than it is given in this paper.

George N. Clements, 'The Sonority Cycle and Syllable Organization'

Clements motivates a hierarchy of consonant sonority, then goes on to show how this hierarchy is not only intuitively plausible, but makes correct predictions about preferred syllable structure. Clements uses data from Tamil to show how the hierarchy can predict preferred 'syllable contacts', that is, constraints between the coda of one syllable and the onset of the next. The question of syllable contact is not often discussed; it would be of interest to see the extent to which syllable contact preferences are adhered to in other languages, and whether such preferences can explain morpho-phonological phenomena occurring at morpheme boundaries.

Jonathan Kaye, 'On the Interaction of Theories of Lexical Phonology and Theories of Phonological Phenomena'

The concept of strata of rules and phonetic representations is central to the theory of Lexical Phonology, although the notion in one form or another

dates back to structuralist phonology. Kaye argues that in the theory of Government Phonology, strata may be dispensed with. The reader who is not familiar with the terminology of Government Phonology may be hard pressed to follow the argumentation, which is unfortunate, since the analyses are interesting in their own right, quite apart from any evidence they offer for Kaye's theory.

Kaye's first example concerns nasalization in French; the problem is to explain why e.g. *non-oppression* 'non-oppression' is pronounced with a nasal vowel in the first syllable, while *nonobstant* 'notwithstanding' is pronounced without a nasalized vowel. Kaye accepts the cyclic application of nasalization (contra earlier claims, such as that of Brame 1972, that cyclic rules crucially involve the assignment of stress). His analysis works depends on the assumption that in French, compositional morphology (in *non-oppression)* implies cyclic rule application, and noncompositional morphology (in *nonobstant)* implies noncyclic phonology. Even more odd is the fact that English regular (but not irregular) inflectional phonology is explicitly given as an example of cyclic morphology, whereas in the Sound Pattern of English (Chomsky and Halle 1968), cyclic phonology was restricted to derivational morphology, and that mostly noncompositional (cf. also Aronoff 1976, chapter six; Mohanan 1986, chapter two).

Kaye's second example concerns *CyV* sequences in Japanese. Such sequences arise morpheme-internally, but not across morpheme boundaries: the sequence *C+yV* is simplified to *C+V*. Rejecting an analysis in which the *y*-deletion rule belongs to a lexical stratum and applies only in derived environments, Kaye argues that morpheme-internal *CyV* sequences consist of a *C* onset followed by a *yV* (light diphthong) nucleus. However, the reader is left wondering why there could not be a suffix *yV* in Japanese which was itself a light diphthong.

The reader will be especially advised to study an exposition of Government Phonology before tackling Kaye's third example, which concerns *yers* in Russian.[2] Simplifying greatly, yers (or 'jers') are deletable vowels; in a word with multiple suffixes, where each suffix contains a yer, the last yer in

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[2] I am indebted to Harry Bochner, Arkady Borovsky, Ursula Doleschal, George Fowler, Bill Idsardi, David Pesetsky, Steve Seegmiller, and Irina Sekerina for their help in understanding the behavior of *yers* in Russian. They do not necessarily agree with my interpretation of Kaye's analysis.

a word is deleted, while all yers to its left are lowered. The situation is more complicated when yers appear in prefixes, since a yer in a prefix may be deleted in addition to the final yer in the suffixes. Pesetsky and Halle had suggested a solution based on cyclic rule application.

Kaye proposes a solution based on properties of phonological government: any vowel, including a yer, which is phonetically overt (pretheoretically, one which is not deleted) is able to govern (pretheoretically, allow the deletion of) a yer to its left. While this solution works for the particular cases Kaye considers, it appears to give the wrong result for words with multiple suffixes: Kaye's approach would delete every odd-numbered yer counting from the right end, rather than deleting only the last one. This is apparently what happened historically, but does not accord with the synchronic grammar, at least given the underlying forms assumed in earlier studies.

Björn Lindblom, 'Phonetic content in phonology'

By far the most common 5-vowel system in languages of the world is [ieaou]; similar statements may be made for languages having some other number of vowels. Among consonantal systems, the situation is not so clear, but languages tend to have symmetrical systems in which the same points of articulation are relevant for each set of stops, for instance. Lindblom argues for a phonetic (perceptual or pronounceability) explanation for these facts, as opposed to an innate mental mechanism (e.g. a theory of features which would make the more common systems simpler in some formal sense).

Lindblom's proposal is illustrated in terms of the growth of a lexicon for some hypothetical language. Given for instance the syllables *[ga]* and *[bi]* in words already in the lexicon, words composed of the syllables *[gi]* and *[ba]* are more likely to be added than some other syllable which incorporates sounds not already in the lexicon. However it is unclear to this reviewer how this applies to natural languages which are not created but are rather learned by a child hearing an already existing language. The child does not have a choice, it seems to me, between adding the syllables *[gi]* and *[do]*: the language he is learning determines this for him.

On page 187, Lindblom claims that his figures 2 and 3 (the references in the text to which are reversed), which refer to consonant and vowel systems of attested natural languages, paint a similar picture for these two classes of segments. Specifically, 'elaborated' segments (e.g. nasalized vowels, or

obstruents with contrastive aspiration) do not appear in systems which contain less than some minimum number of distinct vowels or consonants). Lindblom's claim might be easier to evaluate if he had chosen the same sort of charts for the two cases, but in any case it seems not to be true. While 'elaborated' vowels do not appear in his data in phonological systems containing less than a five or six vowels, 'elaborated' consonants in fact appear in his data in systems with a small number of consonants. What instead seems to be the case is that languages avoid having more than some maximal number of 'simple' (= non-elaborated consonants), a result which is easy to understand: only a certain number of consonants may be distinguished without resorting to 'elaborate' features. At any rate, Lindblom's data is an example of how relatively simple data from field workers can be utilized in typological studies.

Marek Piotrowski, 'Polish yers in non-linear phonology'

Piotrowski attacks the problem of Polish yers (similar to the Russian yers discussed in Kaye's paper). His solution is unique, for while it appears that the presence of yers (or rather, their surface manifestation) is unpredictable, Piotrowski argues that the yers are in fact predictable, being epenthesized by rule. In order to maintain this claim, he must resort to marking certain aspects of prosodic structure—in particular extrasyllabicity—in the underlying (lexical) representations of morphemes. The analysis will be of interest to anyone face with the perennial problem of deciding whether an alternation between the presence and absence of some segment is an instance of deletion or epenthesis.

Daniel Recasens, 'Phonology, and speech production and perception: The case of nasal stops'

This paper looks at three tendencies among phonologies of languages:

(1) Velar and palatal nasals are comparatively uncommon.

(2) The place of articulation of nasals tends to be neutralized in syllable coda position.

(3) The coronal (alveolar or dental) nasal tends to be more subject to phonological processes such as assimilation than does the labial nasal.

Recasens cites explanations for these three facts which are based on the phonetic properties of nasals, rather than on innate properties of the mind or on rule formalisms. In this review, I will focus on point three.

A formalist might attribute the propensity of coronal nasals to undergo assimilation to the fact that [+coronal] is an unmarked feature. That is, a coronal segment in its underlying form has no value for the feature [coronal], while noncoronals are marked [-coronal] underlyingly. If a feature can only link (assimilate) to a neighbor if the latter has no value for that feature, then only coronal nasals can assimilate to the point of articulation of an adjacent consonant by being linked to the neighbor's point of articulation features, because only they have no underlying point of articulation—they are, in a certain sense, not coronal until later in the derivation. (This limitation can, however, be overcome if a consonant's point of articulation features are first unlinked by a separate process, but formalists would consider this to be a separate process.)

Recasens proposes a different explanation for the propensity of coronal nasals to undergo assimilation: coronal nasals are less distinctive phonetically from other nasals than are, say, labial nasals. They are therefore more susceptible to being mistaken for another nasal by the language learner, who will then reconstruct an assimilation process.

The question of phonetic vs. formalist explanations is an important one; much of what is commonly taken as evidence in favor of autosegmental phonology, for instance, might find alternative explanation in the phonetic facts as these relate to historical changes underlying synchronic phonology. It is therefore unfortunate that Recasens does not explicitly contrast his explanations for the properties of nasals with more formalist approaches.

Iggy Roca, 'Constraining extrametricality'

In the course of a whirlwind tour of the stress systems of several languages, Roca points out some difficulties for parametric accounts of stress that make use of extrametricality. Roca's solution is to 'autosegmentalize' two forms of extrametricality, thereby rendering them independent: (1) the exclusion of the first or last syllable of a word from the count of syllables for the stress rules, and (2) the 'lightening' of a heavy syllable at the beginning or end of a word, by ignoring one of its moras.

Stress systems are an area where descriptive linguists have made a genuine contribution, since the facts are relatively straightforward. Theoretical

linguists like Roca have proposed typologies of stress patterns, and it would be of great interest if field linguists should discover a stress pattern that does not fit into this typology. Roca's contribution in this volume, however, is not directed at the descriptive linguist, but at other theoretical linguists; the discussion of individual stress systems is breathtakingly brief. It will therefore be of most interest to those already familiar with the literature on parametric accounts of stress system typologies.

In view of disparaging remarks about typologies made in the past by generative linguists, it may be worth pointing out that the typologies proposed by Roca and others are not merely listings of the stress patterns of the world, but ways of deriving the extant patterns (and hopefully no others) from a smaller number of parameters. The goal, in other words, is not just to display the typology, but to explain it.

Sam Rosenthall, 'Prenasalized stops and feature geometry'

Rosenthall explores the process of prenasalized stop formation in several Bantu languages, arguing that the variations in this process in the different languages can be explained by the theory of underspecification, given certain assumptions about the geometry of features. The variations are said to arise from differing choices in underlying feature values: in one language only [+voiced] obstruents may be underlyingly marked for the feature voice, while in another language only [-voiced] stops will be marked for this feature. As I remark below with regard to van der Hulst's article in this volume, this indeterminacy considerably complicates the task of analysis for the linguist describing a language, as well as for the child learning the language, if the theory is correct.

Harry van der Hulst, 'The phonetic and phonological basis of the Simplex Feature Hypothesis'

In one of the longer papers of this volume, van der Hulst argues for a new theory of phonetic features, then shows how this theory simplifies the analysis of a number of vowel harmony systems.

Van der Hulst's theory of phonetic features is quite similar to that of Government Phonology or Dependency Phonology. A vowel (or presumably a consonant; van der Hulst's article discusses only vowel features) is seen as composed of a governing 'feature' and a dependent 'feature'. I place the term 'feature' in quotes because it is not what is usually thought of as a feature (e.g. [+voiced]), but is rather an actual

vowel. Thus the vowel /e/ might be analyzed as the governing feature *i* plus the dependent feature *a*, symbolized as |i-a|:[3] a cross between /i/ and /a/, in essence.

Unfortunately, matters quickly become complex as the number of features which van der Hulst postulates are not sufficient for more complex vowel systems, so that some vowels may have more than one dependent feature; in the language Kirghiz, the vowel /y/ is analyzed as |a-i-u|. Worse, the same phonetic vowels in different languages might be composed of different features: /e/ is variously represented in van der Hulst's analyses as |i-a|, |i|, |i-a,i|, and |a-i|.

There seems to be no simple way of deciding on the 'correct' feature composition in any given language; van der Hulst relies on phonological considerations (in particular, vowel harmony processes) to decide on the correct analysis for each language. I view this as being a drawback to van der Hulst's approach, not only for the practicing linguist but also for the theory as an explanation of what the child does when learning a language. In the classical generative approach, feature composition was viewed as a given, and the child must learn (1) the underlying form of each morpheme based on the alternations which the morpheme undergoes; and (2) the rules governing alternations based on the alternations which the complete set of morphemes undergoes. If van der Hulst is correct, not only does the child have these tasks but also the task of deciding the feature representation in which lexical items and rules are to be expressed. Any change in this feature representation will mean both the underlying forms of morphemes and the phonological rules must be recomputed. The child's search space would seem to be considerably enlarged. Presumably van der Hulst would counter that this additional task is more than compensated for by simplification in the rule component. Until the principles governing the choice among possible feature representations are clarified, this remains unproven. Also unclear is how the child decides on a feature system in languages which happen to lack the relevant phonological processes, such as vowel harmony. Presumably there would be universal defaults, but van der Hulst does not touch on this.

Given these caveats, this paper will be of particular interest to those working in vowel harmony systems.

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[3] For typographic reasons, I have turned van der Hulst's notation on its side.

Overall Comments

The book is well indexed, and typographical errors are remarkably rare for a volume of this sort. Connecting lines have been omitted from tree diagrams in a few places, but the reader may generally reconstruct them. Among the few typos which may cause confusion are the following. On page 25, the second diagram at the bottom of the page should have an additional line from the first *x* to the *a:*. The reference to Ladefoged and Maddieson (1986) on page 123 is missing from the bibliography for that paper; fortunately, it appears in the bibliography for a different paper on page 76. On page 171, the mid vowels in the diagram for Zulu should be labeled *ɛ* and ɔ to agree with the text. On page 254, the last paragraph should read '...The deletion process identified in (8b)...'

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Reviewed by R. J. Sim
SIL—East Africa Group

Descriptive linguists working in the lesser-known languages of the world—the kind of languages which theoreticians delight to term 'exotic'—often place little priority on reading about English syntax. When the justification for their disregard is the low relevance of the structure of English for the description of the 'exotic' phenomenon of non-Indo-European languages, it is very likely to be misplaced. In fact it is as misplaced as the attitudes of those—often intense theoreticians—who regard the phenomenon of 'exotic' languages, and the task of analyzing and describing them, as unworthy of their attention. The gulf fixed between the two camps is attitudinal rather than real and it is well worth seeking a rapprochement.

Language structures, although superficially very diverse, are not infinitely so, and the attention given to universals over the past 20 or more years should be enough to expose the massive commonality of all human languages. Descriptivists who disregard English syntax for its supposed low relevance deprive themselves of an enriching and rewarding experience.

On that note it is a pleasure to introduce two works on English syntax: a reprint of the 1924 volume by Otto Jespersen, and the two volume modern work by J. D. McCawley. If life is too brief a candle to burn more than a fraction of it in giving attention to English, these two works must rank among the worthwhile candidates.

The Introduction to Jespersen (1860-1943), written by McCawley, must be intended as a selling point with the buyer hoping to have a modern evaluation of the earlier author. McCawley's introduction does just that, speaking appreciatively and informatively about Jespersen's contribution. Thus:

> ...he published works whose striking originality makes them still major sources of insight and information as the twentieth century nears its end. Jespersen was decades ahead of his time...

In twenty-five short chapters he covers a wide range of grammatical concepts, discussing each one judiciously and distinguishing carefully between things that differ. This done by prose description, of course, and not by the formalism of modern theoretical linguistics, which is off-putting to some. As a result, the book can be read through and need not be poured over laboriously!

The edition is a photo reprint of the original and the page has a dated appearance. The typeface is a little tiring to read. But, twenty-five short chapters are ideal for reading day by day for a single month's reading program. Try it!

McCawley's work grew out of two consecutive courses on syntax at the University of Chicago.

Its basic orientation is to a generative approach, but theory is secondary to an understanding of the phenomena themselves. Moreover, McCawley does not present his material as a dogma to be followed by the faithful nor a creed to exclude the heretical. It is a relaxed, interesting discussion in which the value to the reader lies in the concise survey of major issues in English syntax, and in the sometimes thought-provoking positions adopted. For example, see his differentiation of VP-deletion and Gapping as distinct ellipsis phenomena of English (pp. 48, 49).

The more theoretical among us will find ourselves with plenty both to agree and disagree about in McCawley's suggestions. Linguists without a great love of theory will equally find much help in the kind of linguistic argumentation which is used to support one analysis against another. Here, too, the book can simply be read through, formalism not intruding into the discussion too much although there are lots of derivational trees.

McCawley will take longer to read, naturally, since it is twice as long, but together these books are worth dipping into on a lazy day when conscience won't justify a holiday and energy won't allow for much else. ■

Universals: Studies in Indian logic and linguistics. By Frits Staal. Chicago: University of Chicago Press. 1988. 67 pages.

Review by Thomas M. Tehan
Payap University, Chiang Mai, Thailand and SIL—Thailand Group

What do we assume when we study another language? What do we assume when we translate? We assume commonalities of thought and language—logic and expression—that make these activities possible. These commonalities, things that all languages share, are examples of human universals that underlie work in linguistics and translation.

Frits Staal, a Professor of Philosophy and South Asian Languages at the University of California, Berkeley, views the search for universals as 'his original point of departure' into the investigations of Indian thought that led to this book (p. ix). He has produced a fascinating book for those who like to reflect on how people of different cultures think. He describes some of the universals of language and thought that have been highlighted and to some extent confirmed by research about India, where the logic system has largely been independent from Western thought.

The book reproduces a series of articles written by Staal over time, in the original stylesheets and typefaces of the journals as the articles originally appeared. I didn't find this too distracting (although once or twice I wished for larger type); on the whole it added a pleasant variety to the book.

Since the various articles come from different sources, different articles have different tones—some of them are decidedly technical in Sanskrit and symbolic logic; the majority are readable, careful scholarship.

To master some of the articles, readers will find that they need to master a plethora of key Sanskrit terms. However, to benefit from the overall thrust of the book and interact with the most important ideas, the Sanskrit terms (and to some extent logic terms) need only be grasped in general. A glossary would have been helpful.

A few of Staal's terms and concepts would be helpful in understanding the following discussion. A universal is 'any feature, entity, or structure that is shared by all human beings but not by any other animals... In other words... [a] characteristic or defining trait of the human species' (p. 1). His favorite example is language itself. One of the questions of the book is, 'Is logic a universal?' Other universals he proposes include the broadest

concepts, such as man's 'feelings, perceptions, knowledge, a will, an intellect, and other faculties that may be thought of as mental properties' (p. 28), as well as specifics such as negation and contradiction. He also adds substance to claims like 'logical principles must be assumed to underlie some of the semantic universals of language' (p. 39).

Staal uses the terms 'shadowy' and 'substantial' to describe universals. Discussions of universals that propose definitions and speculations with little empirical evidence are shadowy. He views the universals as being more 'substantial' if corroborating evidence can be found by comparing the Western logic tradition with another tradition, in this case that of India (p. 5). In part Staal sees Willard Van Orman Quine's arguments as shadowy, but his own investigations as adding substance to those arguments (passim, cf. p. 36). He also sees the universals of language appearing among all peoples. But logic has 'conditional universality', i.e., logic universals depend on the universals of language and become explicit only where a tradition of logic appears (p. 41).

Parts of the Book

Staal summarizes his plan for the book as follows (p. 5):

> The present study is... concerned with more than *two* [language and logic] questions: it addresses *four* that are closely related, like the four vertices of a tetrahedron:
>
> (1) Are there universals of logic?
> (2) Are there universals of language and/or linguistics?
> (3) What is the nature of Indian logic?
> (4) What is the nature of Indian linguistics?'

Numbers 3 and 4 strive to lend substance to some of the shadowy proposals of numbers 1 and 2.

There are three parts to the book preceded by a 50-page introduction. The introduction is really a substantive essay that integrates the articles in the book. It is a summary and more, for although it appears at the beginning of the book, it was the last to be written. The author intends for his readers to be reading the articles and reviews as they are reading the introduction. Staal invites the reader to integrate the substance and depth of the articles into the introductory summary essay. The introduction serves as a roadmap of, an orientation to, and a critique of the following articles.

Part I. 'Indian logic', contains seven articles.

'Correlations between language and logic in Indian thought' (14 pages) demonstrates that 'although some Indian notions look different than those of other traditions, the difference does not stem from the use of radically different logic'. In examining Sanskrit, he gives a 'substantial' example of universals in both language and logic (p. 18). This article uses considerable symbolic logic and Sanskrit terms.

'Formal structures in Indian logic' (8 pages) is much easier reading, as it addresses those 'who don't know Sanskrit or Indian cultural history' (p. 19). It traces the historical development of logic in India, asserting that logic was so well developed by Panini's time (fourth century B.C.), that it must have been practiced for a long time (p. 74). Perhaps this should have been the first article in order.

'Means of formalization in Indian and western logic' (7 pages) is again demanding of the reader's Sanskrit proficiency and symbolic logic skills. It analyses a long Sanskrit text, confirming and extending the symbolic formalizations that were proposed earlier.

'The theory of definition in Indian logic' (5 pages) examines more Sanskrit texts to demonstrate how both Western mathematical formalizations and Sanskrit expressions are equally as precise. He views this as lending support that 'we are again dealing with features of the human mind that seem to be universal' (p. 21). He states that 'grammar was the first discipline in India which developed scientific technique' (p. 89). The symbolic formulae and Venn diagrams are much less demanding than in some of the earlier articles.

'Contraposition in Indian logic' (16 pages) compares the continuity of Indian logic to that of the West from Aristotle to contemporary logic. Both traditions eventually wrestled adequately with the same issues, in this case the relationship between statements such as: 1) 'if the sun shines, it is warm;' 2) 'if it is not warm, the sun does not shine', and 3) 'if the sun does not shine, it is not warm' (p. 22). Both traditions also came to value economy of expression and thought, striving for the most concise formulations and reducing redundancy, using a relatively small number of elementary concepts to define more complex issues instead of inventing new terms.

'Negation and the law of contradiction in Indian thought: A comparative study' (20 pages) is one of the more crucial articles in the book. Staal explores whether Indian logic is some sort of strange unique system that allows both sides of a contradiction to be considered true at the same time, e.g., X is a horse and not a horse at the same time. He argues that although this mystical inclusiveness has a tradition in Buddhist mystical thought, Indian philosophy and logic handle contradiction in the indicative mood with concepts and precision comparable to Western logic. He also demonstrates how Indian logic has developed much more precise tools for discussing non-indicative sentences (e.g., optative and imperative moods) where negation does not function in such an exclusive manner. This latter precision has given much more structure to such disciplines as law, ethics, and ritual. Staal sees Western scholars' 'discovery' of non-contradiction in Indian thought as actually coming from an irrational, mystical strain in Western thought that is not present in the Indian source.

In discussing contradiction, Staal also discusses the two different types of negation in Indo-European languages: sentence negation (e.g., 'He went' versus 'He didn't go'), and word negation (e.g., 'theistic' versus 'a-theistic'). Some of the confusion in discussing the logics of two systems comes from confusing these two types of negation. Indian logic also developed a distinction between language and meta-language, between contrary and contradictory, and between single and double negation. Staal finishes this article with some speculations and questions about the relationship between a logic system and the language that its speakers employed.

'The concept of *paksa* in Indian logic' (14 pages) strives to correct the tendency to impose a Western framework and prejudices on Indian logic. The previous article had discussed how certain Westerners found illogic and mysticism in Indian logic simply because they desired to find it there. In this article Staal compares the Aristotelian syllogism with *paksa* and Indian terms and arguments. In the past other Westerners have found the Aristotelian form of syllogism there. There are similarities but they are not identical and do have significant differences. Similar things can be said about those who found Kantian parallels in Indian logic. Staal finds Indian logic to be much less abstract and more grounded in reality than Western logic and, thus more 'substantial'.

Perhaps at the risk of falling into the same trap, I would like to suggest another parallel with Western thought that might be explored. Pages 130 ff. discuss how in Indian logic an entity is never regarded in isolation but

always in its situation. Any element is always in relation to its locus; thus an entity is best described as a two-place relation. Staal suggests a formula: A(x,y). This can be interpreted as: an entity A is described by 'x occurs in y' (p. 130). This reminded me of some of the basic notations and tenets of tagmemics, with functions and sets. Saying something is a noun phrase (NP) is not as full a description as saying that the NP is functioning in the slot of the subject in the clause. Perhaps more parallels could be gleaned from comparing the material in these pages with tagmemic theory. Might *paksa* in Indian logic be similar to a tagmeme, a function and slot in a relationship?

Part II. Although the first part was interesting, I found my interest quickening a bit as I began reading Part II, 'Indian linguistics'. This part contains the following articles.

'Euclid and Panini' (18 pages), another crucial article, discusses how the Western thought tradition has drawn many of its approaches to reasoning from the mathematical theorem-proof methods of Euclid. In a similar way, much of East Indian thought can be traced to grammatical methods developed by Panini in his fourth century B.C. description of Sanskrit. This article reads especially easily, even though a footnote warned the reader about the 'highly technical and specialized nature of this paper' (p. 143). Panini and later Indian grammarians/logicians developed a rule-governed grammar. They also highly prized the 'principle of concision', i.e. simple, concise, and economical descriptions. In fact it was said 'that grammarians rejoice over the saving of the length of half a short vowel as over the birth of a son' in formulating and ordering their linguistic rules (p. 154). The Sanskrit 'sentences' became so concise and formulaic, that a knowledge of Sanskrit equips people to read these sentences only as much as a knowledge of English equips one to read symbolic logic formulae and their accompanying discussions.

The next three articles show how grammarians in two different traditions came to the same conclusions about rule ordering, context sensitivity, and other linguistic notions.

'A method of linguistic description: The order of consonants according to Panini' (10 pages) would seem from the topic to be one of the most straightforward of the articles. But in reality, I found it one of the most demanding and even confusing. I never developed more than a vague idea of what an *anubandha* was. A glossary would have helped. This article also does not seem to relate as directly to universals as other articles do. Staal discusses

the rule-ordering of Panini's phonology and argues that the separate discovery of rule-ordering in both Indian and Western science is evidence that it belongs as a feature of universal grammar.

'Context-sensitive rules in Panini' (ten pages) describes some Sandhi rules in modern notation. If you've ever struggled with the rules in a Sanskrit book, you'll be appreciative of the more familiar (modern) representations that Staal gives here.

'Panini tested by Fowler's automaton' (three pages) compares Panini's grammar to finite state grammars, context-free grammars, and context-sensitive phrase-structure grammars.

'Syntactic and semantic relations in Panini' (37 pages) discusses parallels and contrasts with Chomsky's 1960s grammatical theory. Both posited 'surface' and 'deep' structures. And Panini's grammar was not a mere taxonomic system. However, Panini's syntactic rules were not ordered as his phonological rules were. Panini had no intention of designing a generative grammar. Staal also states that in some ways, Panini's grammar rules resemble 'realization rules of the kind encountered in... the stratificational grammar of Lamb' (p. 208). Again, the rules about Sanskrit are easier to assimilate in their modern notation than in the traditional Sanskrit form.

Part III presents seven book reviews authored by Staal. The reviews on the whole are more difficult reading than the preceding material. On top of that, they have smaller type. The reviews allow Staal to introduce a few other key scholars and books that bear on Indian logic. Between these and the bibliographies, it is possible to explore the literature further.

Many of the articles in the book have their own bibliographies, thus there is no comprehensive bibliography for the whole book (other than a moderately inclusive one at the end of the introduction). There is also a comprehensive index of several pages. No appendices are included. ■

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From the Linguistics Coordinator

The plight of 'endangered languages' is attracting increasingly greater attention in the linguistics community these days. Many of the field linguists reading this issue of *Notes on Linguistics* have an ongoing, intimate relationship with the community of speakers of a particular endangered language, and some are, no doubt, situated among the members of such a community as they read these words.

Our colleagues who occupy such a position are passionate proponents of vital language use and maintenance of cultural heritage in the endangered language groups in which they are privileged to participate.

These colleagues are in a unique position to contribute to the growing dialogue in the academic linguistics community on this issue. One such opportunity for contribution, recently publicized on the Internet LINGUIST network, deserves highlighting here:

—David Payne

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Call for Papers

FIELD REPORTS/ENDANGERED LANGUAGES

Proposed as an organized session for the January 1995 Linguistic Society of America (LSA) Annual Meeting in New Orleans

The documentation of languages and language use is a central mission of the discipline. Higher order generalizations about human linguistic competence, communicative competence, and linguistic prehistory all depend on it. It is an urgent mission because overall linguistic diversity is declining drastically. Michael Krauss (in *Language* 68:4-10, 1992) estimates that the 6000 or so languages spoken now may be reduced to below 1000 in as few as a hundred years. Yet, while the regular LSA session categories accommodate certain results of field documentation and description, they still fail to support the enterprise itself, or to provide a forum for its most immediate results and products. This failure tends to diminish awareness of field work and documentation as an ongoing enterprise within the discipline at a time of unprecedented urgency. Worse, it places a heavy or even prohibitive burden on beginning linguists who have made a commitment to the documentation and revitalization of endangered languages, but who, in addition to the demands of field work, must tailor their work to existing session categories if they want to present it all.

Therefore, the LSA's Committee on Endangered Languages is soliciting abstracts for a proposed organized session at the January 1995 LSA Meeting titled 'Field

reports/Endangered languages'. The organizers are Ken Hale (MIT) and Tony Woodbury (U. Texas, Austin). If this session is successful, a similar one will be proposed for the 1996 LSA meeting, with the eventual goal of establishing 'Field reports/Endangered languages' as a self-sustaining regular session category at future meetings. Abstracts are invited on results of recent field work, especially (but not necessarily) on languages that are endangered, including:

- Squibs presenting fact patterns that are interesting or new for a given language or area (e.g. a verb paradigm not noted in earlier descriptions; or tone in a region where tone languages are not expected).

- Descriptions of new phenomena (cf. such past field 'discoveries' as clicks, vowel harmony, echo words, ergativity, whistled speech, ritual registers).

- Presentations of new findings on issues of language endangerment (e.g. the distribution and speaker strength of languages or dialects in a given area, language preservation or revitalization efforts, attitudes toward language death, or the sociolinguistics of endangered language communities).

- Field methodology (e.g. field techniques, dictionary making, natural text collection/representation, speaker census and survey methods, linguist-community cooperation).

- General issues of concern for field workers (e.g. the intellectual roles of linguist and consultant, the responsibility of linguists to the communities in which they work, or the role of field work in linguistic theory).

As in regular LSA meeting sessions, papers will be 15 minutes long with five minutes for discussion. There are eight slots. Please submit abstracts by Tuesday, August 30, to: Tony Woodbury, Dept. of Linguistics; Calhoun Hall 501; University of Texas, Austin, Texas 78712-1196 (phone (512) 471-1701, e-mail acw@emx.cc.utexas.edu). Submitters must be LSA members. Each submission should conform to the guidelines for 15 minute papers in the December 1993 LSA Bulletin and should consist of a completed Abstract Submittal Form (p. 61), a short abstract on the form provided, and a long abstract as specified. Please also include a phone number or e-mail address where you can be reached on Wednesday, Sept. 7. That way you can be notified about your abstract in time for the September 10 deadline for regular LSA abstracts.

Abstracts will be reviewed by a subcommittee of the Committee on Endangered Languages. To the extent possible, the eight abstracts will be chosen so as to represent languages of most or all major world regions, with an emphasis on languages that are endangered; to demonstrate the range of topics possible for LSA 'Field Reports'; to emphasize the work of younger, less established members of the profession, including especially graduate students; and to showcase field results of importance and interest to linguists generally. ■

Mapping semantic relationships in the lexicon using lexical functions

Charles E. Grimes
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1. The notion of ***lexical functions***[1] allows one to systematically explore the meaning of a lexeme within the context of its culturally associated relationships, and to associate a lexeme with the words and phrases with which a native speaker associates it. One can map in the lexicon the emic networks of meaning of a culture as expressed through the language.

The use of lexical functions was pioneered by Mel'chuk and others who noticed that regular relationships of meaning operate in a

---

[1] Also known in the literature as 'lexical relations'. The more widely used term of 'lexical functions' should not be confused with aspects or terminology in Lexical Functional Grammar. The original ground-breaking work by Mel'chuk and colleagues on lexical functions is profound, but dense and algebraic. That literature tended to emulate mathematical formulas in its notation—hence the term 'function'. This paper moves away from algebraic-like formulas in the notation in favor of a little user-friendliness. I have had a growing number of inquiries about my previous article in *Notes on Linguistics* on this topic (C. Grimes 1987). Since that time I have also learned many things by adding several years of experience in compiling the dictionary of Buru (eastern Indonesia), working intensively on dictionaries of Tetun and Roti (also eastern Indonesia), being involved in teaching a number of lexicography seminars, university courses on dictionary making, lexicography conferences, lexicography workshops, and consulting with a number of linguists and anthropologists compiling dictionaries in a number of languages throughout Southeast Asia and the Pacific. This article was originally written while I was a Visitor in the Department of Linguistics at the Research School of Pacific Studies at the Australian National University. The keen interest in lexicography there, along with the chance to participate in seminars, courses, and workshops specifically on that topic make it a rich environment for developing tools and ideas. Perhaps of greatest interest to the readership of *Notes on Linguistics* is that this article is adapted from a section of a more comprehensive field guide to managing lexical data that I am working on with David Coward to accompany his Multi-Dictionary Formatter [MDF] which provides sophisticated semi-automatic formatting for standard format lexical databases such as for those compiled in SHOEBOX. (The user pushes F for 'Format dictionary'). See footnote 3.

different dimension than do structural patterns. The classic example of this kind of relationship is that semantically *drive* relates to *driver* in the same way that *fly* relates to *pilot*, *write* relates to *writer*, and *treat* relates to *doctor*. These are all typically associated as doers of their actions, but note that not all actor nouns use the English *-er* actor suffix on the verb of the action.[2] These pairs of words are related semantically, and using lexical functions helps us explore and record the networks of lexical associations controlled by the native speaker.

Not only does using lexical functions help field linguists systematically record meaning relationships, but it is also easy to learn a core set of common functions and expand from there. Speakers of the language under investigation also seem to find the approach intuitive.

J. Grimes (1992:125) reported a high level of involvement and motivation by language assistants in his work among the Huichol (a Uto-Aztecan language of west-central Mexico described more fully in J. Grimes 1964, 1981):

> The intriguing thing about following the paths defined by lexical functions is that the informants themselves, even when totally unsophisticated by academic standards, have an intuitive grasp of what is going on and become more and more interested. It was not uncommon for me to have Huichol friends who stopped by casually to see what was going on come back a day or two later after having thought of another lexical correlate, or having remembered a form the rest of us had on the tip of our tongue but couldn't quite remember. I have never seen that level of involvement when working on syntax.
>
> Delayed reaction was normal. After we thought we had exhausted the lexical neighborhood of one word and gone on to another, values of other lexical functions of the first word would pop into people's heads. They would interrupt, and we would go back and fill in. We made it a regular procedure to stop every so often and ask each other, "What else?" It was impossible to simply work our way down a list; we were traveling around and back and forth within semantic neighborhoods most of the time.

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[2] Additional actor nouns are also associated with these verbs, but with more specialized senses. For example, *chauffeur, (navy) flyer, author, nurse,* etc.

C. Grimes (1987:25) similarly reported on fieldwork in Buru (a Central Malayo-Polynesian language of the Austronesian family, eastern Indonesia described more fully in C. Grimes 1991):

> We regularly found that after an hour's session with a language helper we would have enough data to keep us working on it for a whole day. Language helpers frequently were not ready to quit when we were, because they were enjoying themselves so much. In many cases, using this system of exploring the language, the following day the language helper would start off adding information he or she had been mulling over from the previous day's session. In one instance, a man whom I would see for only two or three days out of a month whenever I got down to his village, would point out additional information related to what we had explored the month before!

Since that article was written I have had a friend walk for two-and-a-half days through the mountains from his village to mine, to tell me follow-up information about some lexical networks we had been exploring together more than a year before, when I had lived in his village. He thought it was interesting information that I should know.

2. Suggested field markers and format for lexical functions. Many users have difficulty getting started using lexical functons because they do not know how to structure the information. There is often a sense of 'show me what one looks like and I can take it from there'. Standard format (backslash) codes are useful whether one is compiling a lexical database by hand or on computer. This article provides visual examples of how the user would structure the information using a computer tool such as SHOEBOX for formatting and printing through a tool such as MDF.[3]

---

[3] SHOEBOX v2.0 is a computer database program designed specifically for compiling dictionaries in a 640K DOS environment. It has many other applications as well, but provides fast searches through large lexical databases, allows searches and editing of multiple non-adjacent entries, allows searches across a number of different on-line lexical databases, allows the user to build a lexical database from a text corpus through the INTERLINEAR function, and has many other powerful features that assist with the compiling, management and analysis of lexical data. SHOEBOX may be ordered from: International Computer Services, Box 248, Waxhaw, NC 28173 USA. MDF (Multi-Dictionary Formatter) complements SHOEBOX by formatting the SHOEBOX lexical database into publication format for printing through a word

Following is a suggested bundle of field markers used for lexical functions (or a subset of them). They can be inserted as needed in SHOEBOX manually or through the use of a MACRO.[4]

If bundles can be used recursively within a record as needed. Using a limited number of field markers simplifies the formatting for later printing

\lf	[lexical function]
\le	[English gloss of lexeme in \lf field]
\ln	[national language gloss of \lf field]
\lr	[regional language gloss of \lf field]

of a dictionary—all lexical functions are handled in the same way for printing. Using the FILTERS in SHOEBOX provides for powerful search and retrieval possibilities.[5] A format for using \lf field bundles in SHOEBOX is shown in the example in Table 1.

3. A description of selected lexical functions with examples. Following is a brief listing with description of lexical functions used in the *Encyclopedic dictionary of the Buru language* (MS). Additional lexical functions which have been shown to be relevant for languages

---

processor such as WORD-for-DOS (from Microsoft). MDF will soon be available through the same address for SHOEBOX as listed above.

[4] In SHOEBOX version 1.2a or earlier, the Editor Template can be used for this purpose. The second letter of the standard format markers bring a consistent strategy to multilingual field bundles which should facilitate print routines and other utilities developed in one country to be used in another [e = English, n = national language, r = regional language, v = vernacular], such as gloss fields [\ge \gn \gr], definition fields [\dv \de \dn \dr], example fields [\xv \xe \xn \xr], usage fields [\ue \un \ur], etc. In the examples that follow in this article the following codes are used: \lx lexeme, \sd semantic domain, \ps part-of-speech, \ge gloss-English, \gn gloss-national language, \re reversal-English, \rn reversal-national language, \de definition-English, \lf lexical function, \le English gloss of \lf, \ln national language gloss of \lf, \dt date entry last worked on.

[5] For example, a FILTER set up in SHOEBOX as [lf|Ant] allows one to look at all antonym relations in the lexicon.

\lx huma	[Buru]
\sd Ncult; Nhouse	
\ps n	
\ge house	
\re house ; hut ; building ; dwelling	
\de any building or houselike structure for shelter or shade	
\gn rumah	
\rn rumah , gedung ; pondok	
\lf Group = fenlale	
\le village	
\ln kampung	
\lf Part = heset	
\le wall	
\ln dinding	
\lf Part = atet	
\le roof, thatch	
\ln atap	
\lf Part = subu	
\le door	
\ln pintu	
\lf Mat = kau okon	
\le tree bark	
\ln kulit kayu	
\lf Mat = srahen	
\le split bamboo	
\ln bambu	
\lf Spec = hum kolon	
\le garden house, grain bin	
\ln rumah kebun	
\lf Spec = huma endefut	
\le residential house	
\ln rumah tinggal	
\lf Spec = huma braun	
\le meeting house	
\ln baileo, balai desa	
↓	
\dt 9/9/90	

Table 1

like Russian or English, but which I have not yet found to be applicable to Buru, may be found listed and described in the works of Apresyan, Mel'chuk, and Zholkovsky (1970, 1973), Leed and Nakhimovsky (1979), Mel'chuck (1973, 1982, 1989), Mel'chuck and Polguère (1987), Mel'chuk and Zholkovsky (1970, 1984, 1988), or in J. Grimes (1990, 1992, and MS). Applying them to a specific dictionary project and interaction with language assistants using lexical functions is described in C. Grimes (1987). In several cases I have renamed or generalized Mel'chuk's relations, combining several under a single lexical relation for ease of learning and use. Comparable abbreviations in Mel'chuk's or J. Grimes' schema are in square brackets following the description e.g. [≈Syn^].

****

Syn ***Synonym***: Forms substitutable for the headword in most contexts (exact synonyms are rare). [≈ Syn^ (hyperonym), Syn< (less inclusive), Syn > (more inclusive)].

\lx beka
\ge first (before doing something else) **\lf Syn = peni** \le first (before doing something else)

SynD ***Dialectal synonym***: Usually equivalent to headword. Dialect named in \le field.

\lx inhadat
\ge mosquito **\lf Synd = senget** \le Rana, Lisela

SynL ***Loan synonym***: Loans assimilated into everyday speech (common or frequent usage sometimes having adapted to vernacular phonotactics) which are equated with or substitutable for the headword.

\lx ka
\ge habitual aspect **\lf SynL = jaga** \le Ambonese Malay 'habitual'

SynR ***Register synonym***: Synonym in another speech register (as in speech levels of Javanese, Balinese, or Sundanese).

\lx irung [Javanese]
\ge nose **\lf SynR = grana** \le H(igh), Krama Inggil

SynT ***Taboo synonym***: Usually equivalent, but can also have nontaboo range of meaning that is different. Often lexicalized circumlocutions. More localized than **SynD**.

\lx minjangan
\ge deer **\lf SynT = wadun** \le deer (back of neck)

Gen ***Generic (hyperonym)***: Term which is semantically broader than and subsumes headword. Implies a generic-specific relationship, so it should be cross-referenced as a specific under the entry for the generic. These should follow native speaker intuitions about what term the headword clusters under. The generic term should be able to substitute for the specific. [= Gener].

\lx feten
\ge foxtail millet **\lf Gen = agat** \le grain

\lx sgege
\ge carry under-arm **\lf Gen = ego** \le get, take, carry

Spec ***Specific (hyponym)***: Terms which are semantically subsumed under the headword. Types of a kind. The compiler should check to make sure that these follow emic groupings, rather than reflecting the lexicographer's ideas about how native taxonomies 'ought to be'. All of the known specifics should be listed under the entry for the generic term. These generic-specific relationships should be reciprocally cross-referenced. [≈ Spec, Species, Female, Male, Sub-adult, Child].

\lx lata
\ge cut **\lf Spec = bisi** \le carve **\lf Spec = hete** \le cut into sections for use ↓

\lx enhero
\ge spear **\lf Spec = pang.neet** \le six-barbed spear **\lf Spec = pangat goit** \le special spear for killing humans ↓

Sim ***Similar***: Near synonyms or other terms at the same level of native taxonomy that are subsumed under the same generic term and are relevant for clarifying the headword. These terms are often given in describing the headword, saying 'x is like y, but different'.. Normally, the more thorough list of the generic-specific taxonomy should be found under the generic term, rather than listing many Sim under each specific. For Buru, reproducing all 17 cutting verbs under each specific entry is not economical. [≈ Syn^, Syn<, Syn>].

\lx pang neet
\ge six-barbed spear **\lf Sim = pang.paat** \le four-barbed spear

\lx bisi
\ge carve **\lf Sim = dasa** \le cut to a sharp point

Nact ***Actor noun***: Doer of verb, implying habitual or characteristic association. 'Actor' here is in the macrorole sense described by Foley and Van Valin (1984) for core arguments. [≈ S1, N1].[6]

\lx ekfilik
\ge sell **\lf Nact = geb.kaleli** \le merchant

Nug ***Undergoer noun***: Typical undergoer of a verb; the undergoer implied if none specified. 'Undergoer' here is in the macrorole sense described by Foley and Van Valin (1984) for core arguments. [≈ S1, S2, N1, N2].

\lx hete
\ge cut into sections for use **\lf Nug = kau bana** \le firewood

Nloc ***Noun of location***: Location normally associated with headword. [= Nloc].

---

[6] Using Nact, Nug, Ninst, etc., is a different strategy than the N0, N1, N2, N3, used by Mel'chuk and company. I find my current system far more practical for both remembering and for training others to use *lexical functions*.

\lx agat
\ge grain (dried)
\lf Nloc = hum.kolon
\le grain storage house

Ninst ***Instrument noun***: Instrument associated with the action of the headword; the instrument implied if unspecified. [≈ S3, N3, Ninst].

\lx bisi
\ge carve
\lf Ninst = katuen
\le machete

\lx dihi
\ps vt
\ge comb
\lf Ninst = dihit
\le comb (n)

Nben ***Benefactee***: The one who benefits from the activity. The one implied if none specified.

\lx soso
\ps v
\ge nurse
\lf Nben = an.mihan
\le infant

Ngoal ***Noun of goal***: Typical or unspoken goal associated with or implied by headword.

\lx oli
\ge return
\lf Ngoal = huma
\le house, home

Ndev ***Deverbal noun***: [≈ S0, N0].

\lx iko
\ge go **\lf Ndev = enyikut** \le (his/her) going

Res **Result**: Consequence, resulting state or event. [= Res, Conseq].

\lx mata
\ge die **\lf Res = enmata** \le death

Whole ***Noun of the whole***: The whole, of which the headword is a part.

\lx bubu enitu
\ge ridgepole **\lf Whole = huma** \le house, building

\lx maen
\ge handle ; shaft **\lf Whole = enhero** \le spear

Part ***Part of the whole*:**

\lx huma
\ge house **\lf Part = kasa** \le rafter **\lf Part = subu** \le door

Mat ***Material***: Material used to make headword, or material of which it is composed.

\lx atet
\ge thatch **\lf Mat = bia omon** \le sago palm leaves

Vwhole ***Verb of the whole***: [≈ V0].

\lx enyikut
\ps n \ge going **\lf Vwhole = iko** \le go

Serial ***Conventionalized serial constructions*** using headword.

\lx heka
\ge move away quickly **\lf Serial = heka tuha** \le run off with someone or something

Compound ***Lexicalized compounds*** using headword.

\lx heka
\ge move away quickly **\lf Compound = hek.tatak** \le abandon something

Sit ***Situation***: Situations involving headword, or activities typically associated with headword.

\lx epkiki
\ge dance **\lf Sit = pesta kaweng** \le wedding celebration

Prep ***Preparatory activity***:

\lx atet
\ge thatch **\lf Prep = sau atet** \le sew thatch

Phase ***Phases of head***: For example, processes of building, making, growing, time cycles, etc. [≈ Phase, Seq].

\lx ful.timo
\ge east monsoon **\lf Phase = Samsama** \le lunar month around August

Max ***Superlative degree***: Intense or extreme degree of headword. [≈ Super, Magn, Incr, Plus].

\lx bana
\ge fire **\lf Max = pot.haki** \le forest fire

\lx reden
\ge dark **\lf Max = reden tuni walet mite** \le pitch black

Min ***Reduced/diminished degree***: Minimized or decreased state of headword. [≈ Dec].

\lx bage
\ge sleep **\lf Min = bag.leak** \le nap, siesta

Degrad ***Degradatory degree***: Deteriorated or decayed state.

\lx tonal
\ge cuscus (*Phalanger* marsupial) **\lf Degrad = mefu** \le rotten

\lx kau
\ge wood **\lf Degrad = bono** \le decayed

Caus ***Causal***: [≈ Caus, Perm].

\lx emgea
\ps vn \ge embarrassed **\lf Caus = pemgea** \le embarrass someone

Start ***Inceptive***: Initial phase, inceptive, inchoative. [≈ Incep, Prox].

\lx bana
\ge fire **\lf Start = enhewek bana** \le light a fire

Stop ***Cessative***: Final phase. [≈ Fin, Cess, Liqu, State].

\lx dekat
\ge rain **\lf Stop = dekat dere** \le rain lets up

\lx enein
\ge work **\lf Stop = deak** \le stop, rest from activity

Feel ***Sensation of headword***: [≈ Manif, Sympt].

\lx bana
\ge fire **\lf Feel = poto** \le hot

Sound ***Sound*** uttered by or characteristically associated with headword. [≈ Son].

\lx dole
\ge frog **\lf Sound = troo-troo** \le ribet

Cpart ***Counterpart***, complement, or converse (but not antonym). No cultural middle ground or gradation along a process or scale. Concepts like 'more' and 'less' do not apply. For Buru includes male/female, inside/outside names. [≈ Conv, Comp].

\lx kete
\ge parent-in-law **\lf Cpart = emsawan** \le son-in-law, daughter-in-law

Ant ***Antonym***: Opposite extreme of a process or scale. 'More' and 'less' apply. [≈ Anti, Rev].

\lx emhama
\ge light(weight) **\lf Ant = beha** \le heavy (thing) **\lf Ant = emteno** \le heavy (person)

Head ***Head of group***: [≈ Cap, Lead].

\lx noro
\ge kin group **\lf Head = geb.haa** \le local kin group head

Group ***Group or collective of headword***: [≈ Group, Equip, Mult, Organization].

\lx fafu
\ge pig **\lf Group = fafu reren** \le pig herd

\lx geba
\ge person **\lf Group = geba rano** \le crowd of people

\lx uka
\ge bamboo (generic) **\lf Group = uka lale** \le stand of bamboo

Unit ***Single unit of headword***: Single piece or occurrence. [≈ Sing, Indiv].

\lx uka
\ge bamboo **\lf Unit = uka walan** \le bamboo pole **\lf UnitPart = uka kasen** \le section of bamboo

ParS ***Parallelism (same)***: Parallelism attested in, formulaic, ritual or poetic text meaning (in that context) effectively the same as the headword.

\lx saka
\ge up **\lf ParS = lepak** \le go up, ascend

ParD ***Parallelism (different)***: Parallelism attested in formulaic, ritual or poetic text implying a counterpart, opposite or complementary category to the headword. Like Cpart and Ant, but in formulaic language, often with a sense not found in ordinary language.

\lx saka
\ge up **\lf ParD = pao** \le down

\lx supan
\ge morning **\lf ParD = emhawen** \le evening

Fig ***Figurative usage***:

Idiom ***Conventionalized expressions*** using headword.

\lx agat
\ge grain **\lf Idiom = aga lahin** \le inheritance [lit. 'source of grain']

4. Gaining proficiency with lexical functions. For those desiring to use the concept of lexical functions in their own lexicography work, I suggest starting out with a handful of lexical functions that are already familiar, such as Gen, Spec, Part, Whole, and Mat. Work through your current lexicon and apply them where you can. This will help familiarize you with the notation as well as the idea of mapping semantic networks. Regularly check your assumptions about related words with native speakers to ensure that the lexical database reflects emic networks.

Secondly, review this paper periodically, as well as other literature cited in the bibliography to gradually expand the variety of lexical functions you control.

Many lexical functions imply their inverse (see J. Grimes 1990). Thus, generics (weapons) imply specifics (spears), parts (roofs) imply wholes (houses), and antonyms and counterparts imply their converse, and vice versa. The inverse information should be correctly entered in the proper lexical entries.

For those using SHOEBOX to manage their lexical data, the Jump feature <ALT + F6> becomes a powerful tool for both checking and creating entries for the items listed in the lexical functions \lf field, and for copying relevant material to related entries.

5. Alphabetized starter list of lexical functions. The list below is intended to help beginning lexicographers get started and help them with the bulk of what they will find. Those who want to become proficient users of additional lexical functions, including the use of composite relations (e.g. CausIncep of dark = darken [transitive]; IncepN_0 of storm = break) are referred to J. Grimes (MS—available at some SIL branch and school libraries) and other literature cited in the bibliography.

Ant	Antonym
Caus	Causal
Compound	Lexicalized compound using headword not easily handled by other relations
Cpart	Counterpart (complement, conversive)
Degrad	Degraded degree or state
Feel	Feeling or sensation associated with headword
Fig	Figurative usage
Gen	Generic
Group	Collective/group
Head	Head or leader of group
Idiom	Idiom
Mat	Material used to make headword
Max	Superlative degree of headword
Min	Diminished degree of headword
Nact	Actor noun
Nben	Benefactee noun

Ndev	Deverbal noun
Ninst	Instrumental noun
Ngoal	Goal of action
Nloc	Locative noun
Nug	Undergoer noun
ParS	Parallelism representing *Same* as headword
ParD	Parallelism representing *Different* end of scale
Part	Part of headword
Phase	Phase of headword
Prep	Preparatory activity
Res	Consequence or resulting state
Serial	Conventionalized serial verb combination not clearly handled by other relations
Sim	Similar type at same level of hierarchy
Sit	Situation or activity typically associated with headword
Sound	Sound associated with headword
Spec	Specific (kind of, type of, species)
Start	Beginning phase of headword (inceptive)
Stop	Final phase of headword (cessative)
Syn	Synonym (same range of meaning)
SynD	Synonym in another dialect of the same language
SynL	Loan synonym fully assimilated into language
SynR	Synonym in another register of same language
SynT	Taboo synonym
Unit	Single occurrence of headword
Vwhole	Verb of the whole
Whole	Whole of which the headword is a part

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25TH ANNUAL MEETING OF THE NORTH EAST LINGUISTIC SOCIETY

October 14-16, 1994 - University of Pennsylvania

Deadline for submission of abstracts: July 26, 1994

Abstracts should be anonymous, two pages (8-1/2" by 11"), single-spaced, in 12 point type with 1" margins. The second page need not be limited to data and/or references. Send ten copies of the abstract together with a 3" x 5" card showing the title of the paper, the author's address and affiliation, phone number, and e-mail address to:

NELS 25
Linguistics Dept., 619 Williams Hall, University of Pennsylvania
Philadelphia, PA 19104-6305

Questions can be directed to Gene Buckley and Sabine Iatribou at:
nels25@babel.ling.upenn.edu

Preregistration fees are $20 for students, $30 for all others. The preregistration deadline is Sept. 19, 1994. After this date the registration fees will be $30 and $40.

THE INSTITUTE FOR RESEARCH ON COGNITIVE SCIENCE WILL SPONSOR TWO WORKSHOPS ON OCTOBER 13TH, 1994 - the day preceding the start of NELS

Workshop I: Language Change (historical linguistics and change in progress)
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We invite anonymous 1-page abstracts for 20-minute papers on either of these topics. Send 5 copies to 'Language Change Workshop' or 'Language Acquisition Workshop' at the above address, accompanied by a card stating name, title, affiliation and notification (e-)address. Deadline for submission of abstracts for the workshops in Aug. 9. (Note: papers on these topics are welcomed for main session.)

Remarks and Replies

Reflections on Isthmus Zapotec by David Weber

David Thomas
Thailand Group

David Weber's article on Isthmus Zapotec Inflection (*Notes on Linguistics* 64:20-27) was neatly done and I thoroughly agreed with all except his last two sentences.

Elson and Pickett's chart presentation has the advantages of being more concise (one-third of a page instead of three pages) and of highlighting the most important information (the neat simple semantic structure), and is user friendly to the OWL [ordinary working (field) linguist], but, as Weber pointed out, it has the weakness of ignoring the partial form resemblances. The EWP rule presentation has the advantage of showing the partial resemblances in the allomorphs, but has the weaknesses of being a more abstruse and lengthy form and of implying, by focusing on it, an apparently nonexistent relevance in the language beyond the few forms involved here.

The moral: We should be content to list irregular allomorphs in chart form, with prose notes pointing out the partial regularities.

This doesn't result in a neat mathematical presentation, but language, like us, is human, often with regular habits, but sometimes irregular and better learned than reasoned. ■

***NOTES ON LINGUISTICS* BACK ISSUES AVAILABLE:** The International Linguistics Coordinator's office has extra sets of most *Notes on Linguistics* issues from the first issue published in July of 1979 through 1993 - No. 63. These are available for 50¢ each or sets of 20 for $5.00 (plus cost of shipping) in the Key Building, Room 201 or by contacting the *Notes on Linguistics* editor.

Report

25th Annual Conference on African Linguistics at Rutgers University

Pete Unseth
SIL—Ethiopia Branch

The 25th Annual Conference on African Linguistics (ACAL) was held at Rutgers University, New Brunswick, NJ, March 25-27, 1994. About 100 papers were presented, all of them at least touching on African Languages.

The conference was opened with a plenary address by Dr. Ayo Bamgbose of Nigeria, 'Three decades of African linguistic research'. He discussed research on the continent and problems for African linguists, such as access to the current literature and inadequate local publishing opportunities. In discussing research on the continent he said: 'Special mention must be made of the Summer Institute of Linguistics', observing that the organization had done 'sound linguistic research'. He also challenged linguists to produce broad grammars and not just 'fragmented descriptions' with theoretical emphases.

The schedule grouped about 40 papers in syntax (including morphology), 30 in phonology (including eight within the blossoming Optimality framework), about 15 that might be broadly defined as sociolinguistic, and the rest fell under a variety of other categories.

It has seemed to me that African linguists, in general, are more concerned with language in **use** than they are with abstract theory, and that the more sustained and intimate contact a linguist has with African languages, the more they will follow this, as well. This conference reinforced this notion. Most of the papers in sociolinguistics were by Africans. Carol Myers-Scotton and William Samarin (long time Africanists) presented papers in this area as well, and Russell Schuh's phonology paper was on rhythm in Hausa poetry. Food for thought!

SIL was represented by a display of Africa-related books as well as papers presented by Mike Cahill on 'Marking Peak in Kooni', Myles Leitch on Tonal Assignment in Babole', Jim Roberts on 'Non-tonal Floating Features', and Pete Unseth on 'Negation in Majang'.

In a conference like this, there is not enough time to digest the implications of a certain paper before the next paper began. Even so, I received some ideas and suggestions to help me look at my own data from different ways, and perhaps find some solutions.

The atmosphere was generally very helpful and tolerant. A couple of times people (who are struggling to build reputations) clashed, but usually with others of the same mindset; otherwise things were publicly very amicable. The only harsh criticism I personally heard was a private criticism of a newly graduated Ph.D. using a large theoretical apparatus to accomplish a minor function.

My point is, one should not refrain from giving a paper at a conference like this due to his/her having been involved in field linguistics for quite some time, as opposed to being up on the latest version of a theory coming out of the university classroom.

The next ACAL will be at UCLA in March or April of 1995. The following ACAL 1996, will be in Florida, and the 1997 conference may be at Cornell (New York). ■

MID-AMERICA LINGUISTICS CONFERENCE
October 14-15, 1994
Kansas Union, University of Kansas, Lawrence, Kansas

Invited speakers are Stephen Anderson, Johns Hopkins University, and Salikoko Mufwene, University of Chicago. Special sessions on African Languages, African American English, and Creole Languages. Joint meeting with the Conference on Siouan and Caddoan Languages. **Deadline for submission of abstracts is Aug. 29.**
For additional information about program content contact:

Frances Ingemann
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Linguistics Department, Lawrence, KS 66045
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Reviews

Analyse conversationnelle de l'échange réparateur en wobé: Parler wɛɛ de Côte d'Ivoire.
By Inge Egner. 1988. Berne: Peter Lang. 268 pp.

Reviewed by Eddie Arthur
SIL—Côte d'Ivoire Branch

In her forward, the author suggests that this book may be a first in two areas: the first attempt to apply the Geneva model of conversational analysis to a language other than French and the first systematic analysis of conversation in an African language. This is new ground and is likely to prove both stimulating and challenging to those who wish to tread it.

Given that much of the subject matter is likely to be new to the reader the first chapter gives a very welcome introduction to the methodology and materials which underlie the book. First of all Egner explains the concept of the reparatory exchange, *l'échange réparateur*. 'The aim of the reparatory exchange is to make amends for an offense caused by intruding in the personal domain of another person' (p. 20, translation mine).

The simplest reparatory exchange consists of two movements. For instance, if A steps on B's toes:

A: Sorry! (reparation)
B: That's okay. (satisfaction)

More complex exchanges are possible:

A: Have you got the right time, please? (reparation)
B: Five o'clock. (satisfaction)
A: Thank you. (appreciation)
B: You're welcome. (minimization)

The aim of the exchange is to restore the social balance between the participants and the exchange closes when both parties are satisfied.

There is also a brief, (and in my case, very necessary) introduction to the Geneva model of conversational analysis. A short history of the

development of the model is given along with references to various works by Roulet and others.

The Geneva model has three basic aspects: the hierarchical aspect, in which conversations are divided up into constituents in much the same way as sentences can be broken up into clauses, phrases, etc. The various ranks in the hierarchy are the incursion, the transaction, the exchange, the intervention and the speech act. Each rank being made up of constituents from the rank below. The current study is mainly concerned with the exchange and the intervention. The book is full of tree diagrams to represent the hierarchical structure of the conversation which become very complex once they start dealing with interrupted interventions and exchanges embedded in interventions. In following chapters where numerous possible analyses are given for the same few exchanges it is very hard to see the difference between the various diagrams. Those, who like me, find it difficult to read the Semantic Structure Analyses published by SIL's Translation Department will have trouble here. Having said that, I myself cannot think of any better way to represent the data.

The book uses as its source an actual conversation between two men in rural Côte d'Ivoire. A full transcript of the conversation is given in an appendix. The first chapter concludes with a brief description of the participants and their social situation, similar to those found in most linguistic descriptions. In the current work, however, the introduction does not simply set the stage for the analysis to come; the author continually refers back to the setting in order to assess the contextual assumptions of the speakers. The time of year, geographical setting and relative ages of the participants are all important to the analysis.

Chapter two marks the start of the analysis proper, one reparatory exchange is taken from the dialogue. In addition to the four basic units (reparation, satisfaction, appreciation and minimization) this exchange also contains other features. The rest of the chapter is concerned with determining a schema for the reparatory exchange which will adequately explain the presence of these extra elements. Various possible analyses of the exchange are examined in a step by step manner, finally arriving at an explanation which fits the data.

The third chapter continues the analysis by examining the role of various particles. It rapidly becomes clear that these particles cannot be described without reference to the contextual assumptions held by the speakers. Some

of the material in this chapter was recently published in *Notes on Linguistics* (Egner 1993) so those who do not read French can have access to this work.

The fourth and final chapter looks at the phenomenon of echo-exchanges, one participant in the conversation repeating what the other person says, sometimes in the form of a question. Various theories explaining echo-exchanges are examined and found wanting in the face of the Wobé examples. It appears that no one function can completely explain the role of echo-exchanges in Wobé conversation. Among other things, they seem to both signal the start of a new theme in conversation and to close and complete a theme. Those who wish to read a grammatical description of an African language should probably avoid this book (although they will be well served by Egner's Wobé grammar). However, if they are interested in examining why people say what they say, then this book has a good deal of information. For instance, it explains a phenomenon on which I have often remarked in the language I am investigating when I ask people how they are, they will reply, 'No, I'm fine'. What is the purpose of the 'no'? See Chapter 3 for a detailed discussion.

This is not an easy work to read, especially for those for whom French is not their first language. The unfamiliar concepts and terminology combined with a very high information load meant that I would often have to read a page several times before I grasped its meaning. This being said, the author deserves credit for including regular summaries of her argument throughout the book, which break up the flow and allows the reader to catch his or her breath. I sometimes cheated and read the summary first—this helped with some of the harder parts.

There is a sense, too, in which a high information load is unavoidable in this sort of book. Egner set out to demonstrate the application of a particular model to a set of data. She does so with rigor. Hypotheses are erected and tested against the data, supporting arguments are pulled in from elsewhere and finally conclusions are drawn. Perhaps the greatest value of this book lies in it being an exemplary demonstration of the application of scientific method in a linguistic context.

I came to this book seeking information on a language related to the one which I investigate. It is not going to help me write a reference grammar, but it does give me a lot of food for thought at a discourse level. I'll leave it to the specialists to discuss its merit in terms of advancing the field of

conversational analysis. To the nonexpert, the best reason to read this book is to see a demonstration of how scientific research should be done.

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The phonology of tone: The representation of tonal register. Harry van der Hulst and Keith Snider, eds. 1992. New York: Mouton de Gruyter. Cloth $138.00. 278 pp.

Reviewed by Mike Cahill
SIL/GILLBT (Ghana)

1. Introduction. Be forewarned; this is not a book for the beginner in tone theory. It assumes a fair amount of previous knowledge. In this review I will also assume at least a cursory knowledge of autosegmental theory, but will provide explanation of some terms for the reader's convenience.

Major advances have been made in the understanding of tone in the last several years, building on the autosegmental approach first expounded by Goldsmith (1976). However, there are conceptual problems that continue to be controversial. The articles in this book, which all focus on the matter of tonal register, deal with three of these in particular: the formal representations of multiple tone heights, of contour tones, and of downstep and upstep. Most of the articles focus on African languages in which most recent theoretical advances have dealt—Yip's and Ladd's articles being the exceptions. The articles of the book, which I will discuss are:

Introduction—Keith Snider and Harry van der Hulst
Representation of downstep in Dschang Bamileke—Mary M. Clark
Register tones and tonal geometry—Larry M. Hyman
In defense of a metrical theory of intonational downstep—Robert D. Ladd
Spreading and downstep: Prosodic government in tone languages—Victor Manfredi
Dschang and Ebrie as Akan-type total downstep languages—John M. Stewart
Tonal register in East Asian languages—Moira Yip

In the introductory article, van der Hulst (Leiden University) and Keith Snider (SIL—Cameroon) summarize the issues involved, as well as giving Snider's approach to the problems of downstep and upstep. Then they give a brief synopsis of each of the other papers. These summaries are a valuable preview of the papers, extracting the essentials and putting them side by side.

The first issue has to do with how to represent different heights of tone using tonal features. Many tonal languages have two distinctive levels of tone—high and low. Some languages also have a mid tone. A few have four distinctive levels of tone, and a handful have five levels (e.g. Trique;

see Longacre 1952). If we use two tonal features, e.g. [High] and [Upper], four combinations are possible[1], allowing for up to four levels of tone:

(1)	[+High, +Upper]	High tone	[+High, -Upper]	Mid tone 1
	[-High -Upper]	Low tone	[-High, +Upper]	Mid tone 2

This inventory also makes it possible to distinguish between languages with mid tones in which the mid tone behaves more like a high tone (as far as undergoing downstep, for example), and those in which the mid behaves more like a low tone (e.g. causing downstep).

The problem comes in trying to account for languages with five level tones. It is possible to add another tonal feature, say [Central], but this gives eight possibilities which by far overgenerates what is needed. No language has been claimed to have even six levels of tone, let alone eight.

The second issue is how to represent contour tones. In many African languages, rising and falling tones can be shown to be the result of a sequence of Low-High and High-Low. One advantage of an autosegmental representation is that it allows many-to-one or one-to-many mappings of tone and tone-bearing units (TBUs). Thus a falling tone on a short vowel is represented as in (2):

(2) High Low

These African-type contours can be called *composite contours*, since they are made up of more than one part.

However, in many Asian languages, contour tones behave as units rather than sequences. For example, they spread or replicate as a unit, rather than just one of their composite parts. We can call these *unitary contours*; they spread or replicate as a unit and occur freely on any TBU.

The question is, what representation can best deal with both composite and unitary contours. Several linguists recently have taken advantage of the notion of feature geometry (Clements 1985) and extended it to tonal

---

[1] Note that if [High] and [Low] were used, the combination [+High],[+Low] is logically not allowed, and it would be possible to represent only three levels of tone.

matters. So instead of tonal features being directly linked to the vowel, there are two tonal tiers linked to a tonal node, which is itself linked to the vowel:

(3)

The representation on the left would be a composite contour tone, with two tonal nodes associated to the vowel. The representation on the right could be a unitary contour. There are two tones on the tonal tier, but there is only one tonal node associated with the vowel, so it can act as a unit.

The third issue is the matter of tonal register. In many tonal languages, especially African ones, the phenomena of downstep and downdrift exist. In downdrift, when there is a High-Low-High tone sequence, the second High is phonetically at a lower pitch than the first:

(4)

Thereafter, the whole 'register' is reset, and no High tone rises higher than that second high. Thus 'downdrift' resets the register every time it occurs, and can occur several times in the same utterance:

(5) Kófí pàpá rèkasá 'Kofi's father is speaking'.
(Akan: Akuapim and Asante dialects; data from Dolphyne 1988:57)

Downstep is closely related. In downstep there are two adjacent high tones, but the second is phonetically lower than the first, commonly by the same amount as in the downdrift phenomenon. Historically or synchronically, downstep in many languages can be linked to a deleted low tone (Clements and Ford 1979). Many working within an autosegmental framework have assumed that all downstep is the result of a floating low tone, unpronounced but still affecting the pitch of a following high tone. In this view, downstep and downdrift have the same cause—a low tone before a high. Since the register lowering can occur several times, the common assumption is that it is not a binary factor, thus cannot be represented by a binary phonological

feature and must be dealt with in the phonetics rather than the phonological component in a rule such as:

(6) $X^i \rightarrow X^{i+1}$ / [High][Low]__

where X^i is the pitch register and X^{i+1} lowers the register by one factor.

A potential problem here is the existence in some languages of 'upstep' as well as downstep. The tonal register can not only go down, but up as well. Also, for some languages, e.g. KiShambaa (Odden 1982), down-step cannot be linked to any floating low tone.

2 Keith Snider—'A register tier model'. Snider does not have a full article in this volume but in the introductory chapter sketches his approach to the representation of tonal register. Since downstep is cumulative and nonbinary, the assumption of many linguists has been that it is best dealt with in the phonetic component as mentioned above but Snider, here and more fully in Snider (1990), argues that it is a phonological process. He proposes a Modal Tier for representing the levels of tone, and a Register Tier to indicate shifts of register, both attached to a Tonal Node, which links to the TBU:

(7)

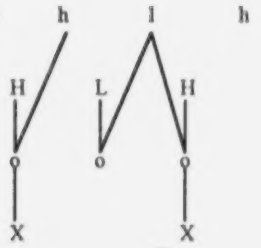
Register Tier
Modal Tier
Tonal Node
TBU

(adapted from p. 11)

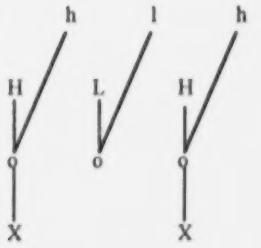
In (7), there is one H tone linked to two TBUs. However, the first TBU is linked to a **h** register, and the second TBU to a **l** register. Thus the second TBU's H tone is downstepped with respect to the first.

Both downstep and upstep are often connected with floating low tones, and Snider's model accounts for this straightforwardly:

(8) (a)



(b)



(adapted from p. 14)

In (8a), the l from the Register Tier on the floating Low tone has spread to the following High tone, delinking the h. The result is that the second High tone is manifested on a lower register than the first High; it is downstepped. In (8b), there is the same tonal configuration but here the l has not spread. In this model, floating tones (whether H, L, h, or l) have no direct phonetic manifestation. The second High tone, since it is linked to a second h, is upstepped with respect to the first.

Snider's model can handle up to four discrete tone levels by combining the features of the Modal and Tonal Register Tiers.

3. Mary Clark—'Representation of downstep in Dschang Bamileke'. Clark briefly reviews three approaches to formalizing downstep which have great shortcomings in her view. The first is postulating a separate tier for tonal register which she shows is not needed for Hausa. Second, she shows that a floating low tone being responsible for downstep does not work in a number of languages (Kishambaa, Supyire, and Dschang). Third, she mentions some theoretical problems with Hyman's approach to Dschang, though her solution builds on his.

Clark's solution to the representation of downstep starts with two tonal features, [Upper] and [Raised], which are both attached to a tonal node. Her contention is that downstep in Dschang is realized by a phonetic-implementation rule:

(9) $Xi \rightarrow Xi+1 / [\alpha T]__[\alpha T]$ (from p. 66)

where Xi is a tone register and Xi+1 is one register lower. Note that her formulation crucially depends on suspending the OCP[2]; downstep occurs when two identical tones are adjacent.

To get to this last conclusion, Clark analyzes the associative construction[3] of Dschang Bamileke in detail (data from Hyman 1985). One noteworthy thing about Dschang is that low tones can be downstepped as well as high tones. Also, Hyman says that there are 'double downsteps' at some points. Clark accounts for all these in the associative construction with several rules that interact in different lexical strata to give the proper outputs. At points they seem a little labored, but they work. She also gave a summary of the rules and the lexical levels at which they apply, which I appreciated.

Clark's article was well-written and well-argued but limited in scope. She shows that a phonetic-implementation rule as above, is a superior solution to represent downstep in at least some languages. Upstep does not fit into her scheme.

4. Larry Hyman—'Register tones and tonal geometry'. Hyman's goal is a tonal geometry that will account for not only the common phenomenon of downstepped high tones, but also the less common downstepped mid and low tones, as well as upstepped highs. He adapts Yip's (1989) tonal geometry as follows:

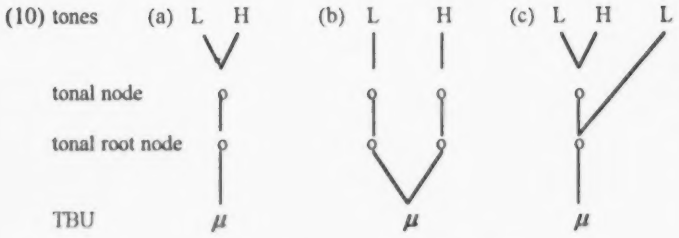
---

[2] OCP = Obligatory Contour Principle. This principle states that when two identical autosegments are adjacent, they will merge into one, e.g.:

```
H   H       H
|   |   →   /\
V   V       V V
```

or that underlying representations should always be of the nature of that to the right of the arrow, and not like that to the left of the arrow. The OCP seems to be common but by no means universal in languages.

[3] An associative construction is one in which two nouns are associated together, e.g. 'book of linguists'. This of construction can be thought of as a type of 'possessive' relation in everyday terms.



(adapted from pp. 81, 83)

Above, when L and H are associated to the tonal node, they define tone levels. Thus a L would indicate low tone, H high tone, and either HL or LH, as in (a) defines a mid tone. A contour tone is symbolized as in (b), where the L and H is each attached to the TBU by its own tonal root node. The function of the tonal root node is to indicate register raising or lowering. So in (c), the L and H attached to the tonal node indicate a mid tone, and the other L attached to the tonal root node indicate a downstep. Thus (c) indicates a downstepped mid tone—not common, but attested.

A distinctive of Hyman's proposal is to use the same features H and L to indicate both tone levels and shift register. Using the same feature for two different functions, and even allowing the feature to associate to two different nodes, is not something with which many linguists would be comfortable.

Hyman devotes the majority of his paper showing how his system accounts for upstepped high tones in Engenni, Mankon, and Kirimi, each of which raises a high tone before a low one.

Hyman's system allows for many different assimilatory processes, and works very nicely for the languages with which he deals. In its present form, his model does not account for languages with four or five tone levels.

5. Robert D. Ladd—'In defense of a metrical theory of intonational downstep'. Ladd's paper is unique in this volume in that it does not deal with tonal languages but with the intonation of languages such as English and Japanese. He deals with much different issues than the others. He evidently has a running controversy with Pierrehumbert and Beckman; more of his paper is devoted to criticizing their approach than to positively expounding his own.

English optionally has what can be called downstep between constituents with individual pitch accents:

(11)

'There are many intermediate levels'.
H* +L H*+L H*+L

Pierrehumbert and Beckman (1988) treat these as linear sequences of tone, somewhat similar to classical downstep in tonal languages, with a L tone between two H tones causing the second H to be downstepped, as above. Ladd, on the other hand, maintains these register shifts are controlled by 'register trees', abstract prosodic structures in which the relative heights of prosodic constituents are specified:

(12)

phrase level	h		l
	/ \		/ \
accent/tone level	h l		h l
	| |		| |
	T T		T T

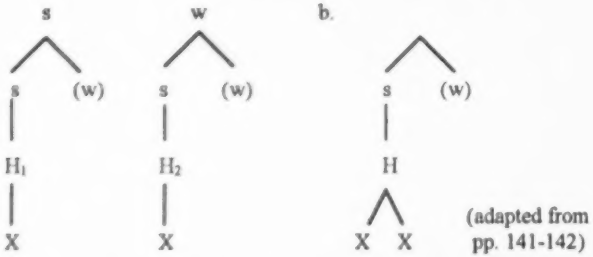
The advantage of these trees is that it accounts for 'declination within declination', that is, downward trends in pitch within a phrase, and also a downward trend from one phrase to the next.

6. Victor Manfredi—'Spreading and downstep: Prosodic government in tone languages'. The editors of this volume deliberately sought a variety of theoretical viewpoints. Manfredi is the only writer included here who advocates a metrical, rather than strictly autosegmental, approach to tone. He criticizes a rule-based approach as unnecessarily *ad hoc*, always needing diacritics on lexical items and/or rules, and maintains that by setting the proper principles and parameters for an individual language, rules can be avoided altogether. This approach is termed 'charm-and-government phonology'.

Manfredi borrows terminology and concepts from Chomsky's Government and Binding syntax model, and contends that a syntactical analysis is necessary for correct phonological derivations. For example, he shows (pp. 152-153) that Hyman's 'minimal tone pairs' in Dschang-Bamileke have very different morphemic structures, and Manfredi's re-analysis is able to avoid the floating tones Hyman uses.

Manfredi writes that 'downstep is a relation between metrical feet'.[4] In (13), each H projects an **s** node which automatically entails a **b** following it. The **w** may have a tone also, though the examples below do not:

(13) a. b.



(adapted from pp. 141-142)

(13a) is a representation of downstep. Manfredi's application of the OCP means that if two H tones are phonetically distinct (as in downstep), they must belong to different feet and so are not strictly adjacent. On the other hand, if two TBU's have the same pitch for H, they must belong to the same foot and the same H tone, as in (13b).

Manfredi thus supports the OCP (unlike Clark), and analyzes various nominal and verbal constructions of Igbo and the associative construction of *ɣɔmalá?-Yamba* nouns. He crucially depends on his syntactic analysis which previous rule-based accounts did not deal with.

Manfredi does not deal with the representation of more than two tone levels or contour tones. Also, Manfredi's use of nonstandard language names may be a bit confusing. He uses *ɣekoyó* instead of the well-known *Kikuyu* (cf. Clements and Ford 1979), and *ɣɔmalá?-Yamba* for *Bamileke Dschang* (cf. Hyman 1985).

---

[4] In metrical phonology, syllables are most commonly grouped into 'feet' of two syllables each, one being more prominent than the other. The more prominent one is labeled **s** for 'strong'; the less prominent labeled **b** for 'weak'.

7. John M. Stewart—'Dschang and Ebrié as Akan-type total downstep languages'. Stewart's subject in this paper is register tone—downstep and upstep. He shows that Ebrié and Dschang share many tonal characteristics which are similar to Akan while lacking one significant feature.

Akan is a classical downstepping language with downstep closely related to downdrift (what Stewart terms 'automatic downstep'). Dschang has downstep but not downdrift. Ebrié has three discrete level tones and no downdrift but the mid tone can be analyzed as a downstepped high.

In Stewart's model there are four tonal autosegments: H and L being 'linking tones' which may be linked or floating; and h and l, which are never linked and are realized as downstep and upstep respectively. Of the three languages under discussion, Akan lacks the h autosegment; it has no upstepping.

The three languages do have striking similarities when viewed in Stewart's model. They all have 'automatic downstep', which is HlL to the exclusion of HL. They all eliminate these inadmissible HL sequences, at word boundaries for instance, by means of a Low tone stepping rule which changes HL to Hl.

Ebrié and Dschang are also seen to be very similar in that they have at least five identical or near-identical tone rules. They also both have 'automatic upstep', that is LhH to the exclusion of LH. In Ebrié, upstep is paired with downstep so that the H and L tones are realized on three discrete levels.

Stewart supports his proposal with extensive analysis of all three languages (his article is the longest in the volume—59 pages). He ends with an interesting historical proposal. With Akan's downstepping, it has an infinite (in principle) number of surface levels. Ebrié has three surface levels. These are the two most common type of tone systems found in West Africa. Stewart tentatively suggests that Ebrié type systems develop from Akan-type systems by adding upstep to reduce the number of surface levels. The plausibility of this is increased if we accept the commonly held idea that Akan-type systems themselves developed from simple two-tone systems by adding downstep. If so, then upstep would fill the gap created by having downstep alone.

8. Moira Yip—'Tonal register in East Asian languages'. When Africanists use the term 'register', the concept is of a limited range of pitch

within which the tones are manifested. Downstep lowers the register while upstep raises it. However, Yip reminds us that 'register' is used in other ways as well. She lists and investigates the relation between 'tonal register', 'phonation register', and 'intonation register', the last term corresponding to the 'register' of other articles in this volume.

'Tonal register' divides the pitch of the voice into two ranges called [+upper] register and [-upper] register. This has to do with how tone levels and contours are represented. Most work on Chinese tone denotes pitch by numbers, five being high and one low, with contour tones denoted by their starting and ending points. Yip subdivides the pitch range registers by the subsidiary feature [raised] so level tones may be represented thus:

(14)

[+upper]	[+upper]	[-upper]	[-upper]
|	|	|	|
[+raised]	[-raised]	[+raised]	[-raised]
55	44 or 33	33 or 22	11

Contour tones have a sequence of two specifications for [raised], shown below with H and L used as shorthand for [+raised] and [-raised].

(15)

[+upper]	[+upper]	[-upper]	[-upper]
/ \	/ \	/ \	/ \
L H	H L	L H	H L
35	53	13	31

Yip shows in Taiwanese and Suzhou that the subsidiary feature [raised] can be deleted with the [upper] register being unaffected. Similarly, in Fuzhou the register feature [upper] can spread, leaving the tones unaffected. This supports the representation in (14-15).

'Phonation register' has to do with whether a vowel is creaky or murmured (for those languages which have such distinctions). This can affect the pitch of a syllable, and Yip shows in Shanghai that in certain contexts both murmur and tone contrasts disappear, suggesting murmur is dominated by a tonal tier. However, in Tibetan, murmur and all laryngeal contrasts disappear, suggesting that murmur is a laryngeal feature. There is a historical relationship between tone and laryngeal features, and Yip suggests that in at least some languages the feature [murmur] is attached to both the tonal root node and the laryngeal node (assuming feature geometry). This is a radical departure from standard representations, but may explain some languages as being in transition to becoming a totally tonal language.

'Intonation register' determines the level on which a lexical tone is actually realized in an utterance. Yip departs from Asian languages to cite the case of Hausa (African) and compare with Chinese languages. 'Register' in Hausa resets the pitch range for the whole utterance, while tonal register in Chinese affects only one syllable. Also, a lexical Low tone in Hausa is closely tied to a lowering of the intonation register, while in Chinese languages this is not the case. Yip concludes that intonational effects in Chinese must include information from all tonal features, not only her register tier [upper].

Yip also concludes that though the three types of register are related, they cannot be reduced to a single feature.

9. Concluding remarks. One might wonder if there is ever going to be a system or theory that accounts for all tonal phenomena in every language, or even every African language. We continually run into idiosyncrasies like the 'total downstep' cited by Manfredi (which lowers a H tone to the level of a preceding L), or the 'double downstep' cited by Clark.

Another observation is that everyone is dealing with the same core of data. Hyman's 1985 article was the takeoff point for almost everyone, and with good reason. In it (pp. 50-51), Hyman has 72 well-selected examples of tonal combinations with the associative construction. The perturbations are complex and he gives an example of every combination possible. I would hope that at least some of the articles in 'Tone in Five Languages of Cameroon', published by SIL, would also be frequently cited for the same reason—some are particularly 'data-rich'.

'The Phonology of Tone' has indices for languages, authors, and subjects, all of which combine to increase the volume's usefulness. The references at the end of each article are also helpful for those seeking more information on either language data or theory. There are few misprints. The volume is well-bound and should be quite durable. For the person who is interested in tone theory, this would be a good acquisition.

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A method of language sampling. By Jan Rijkhoff, Dik Bakker, Kees Hengeveld, Peter Kahrel. 1993. Studies in language 17:169-203.

Reviewed by Irvine Davis
SIL—North America Branch

The concern of the authors here is the opposite of that of many comparative linguists. Rather than proposing a method for identifying languages that are closely related genetically, the authors are concerned with obtaining samples of languages that will be optimally diverse. This concern stems from interest in the study of linguistic universals and the goal of formulating a general theory of grammar that is true of all languages and not just of those belonging to a particular language family. To recognize what are truly linguistic universals, one must of course base his conclusions on the broadest possible sampling of diverse languages.

The authors recognize that, in light of recent proposals of very broad groupings of genetically related languages, it is increasingly difficult to obtain language samplings that will include only languages that are likely to be unrelated genetically. The authors accept Meritt Ruhlen's 1987 classification of the world's languages (*A guide to the world's languages, volume 1: Classification.* London: Edward Arnold). If one were to choose a sampling of languages in which no two were of the same phylum based on this classification, he would be limited to less than thirty languages—a sample too small for serious conclusions regarding linguistic universals.

The sampling method proposed by the authors is summarized as follows:

1) The universe from which the sample is taken contains all known extant and extinct languages.

2) All phyla are represented in the sample by at least one member.

3) Additional languages are selected on the basis of the so-called Diversity Value of a phylum.

4) The Diversity Value of a phylum is determined on the basis of an objective measure.

A detailed discussion follows concerning how this Diversity Value is arrived at on the basis of the internal structure of the genetic language tree of that phylum. In essence, it is based on the number of major branches of

the family tree rather than on the total number of languages of the phylum. Details are given on how to arrive at a 100-language sample.

The whole procedure, of course, assumes that Ruhlen's classification is accurate and that we really know how the languages of the world group into phyla and divide into subphyla, language families, etc. In light of the present state of the art and the differing classifications proposed by various authors, it must be concluded that the sampling method outlined in this article, although an improvement over previously proposed methods, falls short of the high degree of accuracy suggested by the elaborate formulae and charts presented.

(Note: This review was reprinted with the author's permission from *Academic News and Reviews* of the North America Branch of SIL.) ■

2ND INTERNATIONAL CONFERENCE ON OCEANIC LINGUISTICS
Suva, Fiji - July 3-7, 1995
Co-organized by Department of Literature and Language of USP and Institute of Fijian Language and Culture (Ministry of Fijian Affairs)

LASSO XXIII
23RD ANNUAL MEETING OF LINGUISTIC ASSOCIATION OF THE SOUTHWEST
October 21-23, 1994 - Houston, Texas
Invited Speaker: Ian Hancock, University of Texas-Austin

Deadline for receipt of abstracts (June 15) was passed before scheduled publication of this issue of *Notes on Linguistics*. For more information contact:
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Lexical matters. Edited by Ivan A. Sag and Anna Szabolcsi. 1992. Stanford: Center for the Study of Language and Information (Lecture Notes No. 24). Pp. 360. Cloth $49.95, Paper $19.95.

Reviewed by Karl J. Franklin
South Pacific SIL—Kangaroo Ground, Australia

This is a brief review on the book, *Lexical Matters*, edited by Sag and Szabolcsi, that outlines for average field linguists some chapters which they might want to avoid, and offers some comments on the remaining ones which suggest that they are somewhat dense as well. The book provokes me to maintain a sense of distance from 'pure' theoretical linguistics and to concentrate on what is actually useful to people who live outside of universities.

I found the following chapters to be among the least useful:

> 'Thematic relations as links between nominal reference and temporal constitution' (chapter 2 by M. Krifka).
>
> 'Complex predicates and morpholexical relatedness: locative alternation in Hungarian' (chapter 3 by F. Ackerman).
>
> 'Focus-based inferences in sentence comprehension' (chapter 8 by G. Gergely).
>
> 'Combinatory grammar and projection from the lexicon' (chapter 9 by A. Szabolcsi).

The language and symbolism in these chapters are extremely complex and tightly packed.

There are some conclusions, though, reached by authors of the remaining articles which are somewhat more readable. However, as you investigate them, try to read something from the *Far Side* between each chapter.

C. Tenny (chapter 1, 'The aspectual interface hypothesis') discusses possible and impossible verbs:

> A theory proposing a correlation between aspectual and syntactic structure makes strong predictions about possible and impossible verbs. If the view advanced in this paper is correct, verbs that violate aspectual constraints on argument structure should be impossible verbs.

Unfortunately Tenny concludes:

> This kind of prediction about possible or impossible verbs may be testable in language acquisition experiments. I know of no experimental work that sheds light on them, and the appropriate tests may be difficult to design (pp. 24-25).

D. F. Farkas (chapter 4, 'On obviation') comments on 'why, in many languages, the subject of certain subordinate clauses must be disjoint in reference from certain NPs in the immediately higher clause...' This is a problem that is of immediate interest to the GB practitioners, and the conclusion Farkas gives is that 'obviation is the effect of a blocking mechanism that limits the choice between two complement types subcategorized or by a verb' (p. 106).

Other insights on blocking in this volume can be found in W. J. Poser (chapter 5, 'Blocking of phrasal constructions by lexical items'):

> Blocking is the widely observed phenomenon where the existence of one form prevents the creation and use of another form that would otherwise be expected to occur... (p. 111).

M. Liberman and R. Sproat (chapter 6, 'The stress and structure of modified noun phrases in English') produce the chapter which is the most readable by the average field linguist not steeped in the particulars of the theory at hand. It is directed toward establishing 'an adequate set of descriptive categories, able to support a formal model of syntax, semantics and prosody of complex nominals' (p. 131). They claim that this model will be 'adequate for parsing and assigning stress to modified noun phrases in unconstrained English test'.

On the topic of the 'lexical entailment theory of control', P. Jacobson (chapter 10, 'The lexical entailment theory of control and the so-called tough-construction') gives an account of the control theory in intricate examples and arguments on the syntax of the tough-construction (sentences that include forms like 'impossible' and 'hard').

Finally, I. Sag, L. Karttunen, and J. Goldberg have written a chapter called 'A lexical analysis of Icelandic case' (chapter 11), or what they call the problem of the 'quirky' case in Icelandic. What is gained is:

> As complexity of syntactic rules is continually eliminated in favor of more complex lexical entries, the development of such methods for achieving lexical compaction is not just a desideratum, it is necessary (p. 317).

As I implied earlier, it's tough reading. The title of the book (*Lexical Matters*) sounds innocuous enough, but then you can't tell a book by its cover. ■

English Grammar: An outline. By Rodney Huddleston. 1988. Cambridge: Cambridge University Press. 225 pp. Hardback $39.50, Paperback $14.95.

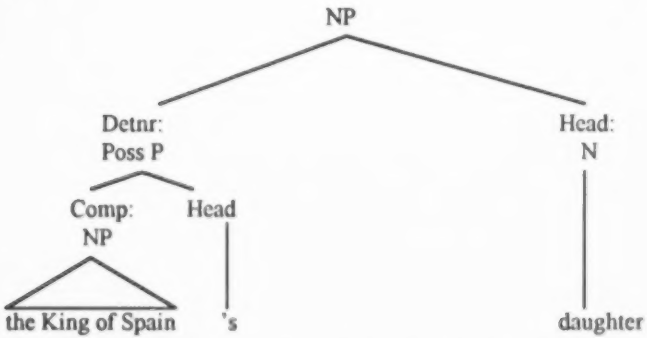
Reviewed by Ron Olson and Michelle Olson.
SIL—Members-in-Training

In the comprehensive book, *English Grammar: An outline*, Rodney Huddleston presents the English language from a descriptive point of view. As a necessary part of his description, Huddleston includes the prescriptive grammar that is taught in U.S. schools and categorizes this as a brand of formal English grammar that is used in certain contexts (p. 19). Huddleston has adapted terms from transformational-generative grammar to fit a more informal framework for this book (p. 11). The book is intended as an introductory English textbook at the tertiary level. However, the book can have many other uses which Huddleston also suggests in the preface: 'an elementary course on English within a linguistics programme... for any course aiming to present a descriptive overview of the structure of English' (p. ix).

The book is divided into two major sections. Chapters 2-8 focus on kernel clauses, and chapters 9-13 on nonkernel clauses. Chapters one and two give an overview of the English language with linguistic terms and the parts of speech defined and explained. There are no summary chapters but each chapter has a summary in the form of written exercises.

Chapter one begins by defining terms, then using these terms, carefully building upon them to define more terms until he is able to use these terms to describe the English language. In chapter two Huddleston explains the parts of speech using the linguistic terms he has introduced in chapter one, then by showing how the parts of speech compare, contrast, and interrelate with one another. For example, on pages 33-34, English clitics are clearly explained, complete with a tree to illustrate. Huddleston defines a clitic as 'something which from a grammatical point of view is a word but which

merges phonologically with an adjacent word as it lacks itself the normal phonological properties of a word...' (p. 34). Then Huddleston uses the phrase, *the King of Spain's daughter,* to show the possessive 's as a clitic in English, 'a word, not an affix, [because] it enters into construction with a phrase, not a stem' (p. 34). He shows the 's as the head in the possessive phrase:



Following chapter two, Huddleston begins a detailed discussion of the parts of speech, inflections, and how these function in kernel clauses.

Chapter three entitled 'Verbs' defines a verb in any language as 'having the following properties:'

> (a) It contains amongst its most central members the morphologically simplest words denoting actions, processes or events; in predications of these types at least, the word functioning as head of the predicate expression will normally belong to the class we call verb.
>
> (b) Members of the class carry inflections of tense, aspect and mood if the language has these as inflectional categories (p. 37).

Huddleston continues on with a general description of verbs, then explains how verbs function in the English language in sections on inflectional paradigms, morphology, and categories, sections on resolution of syncretism, verb inflection, finiteness and clause subordination (p. 43), and finally a section on Operators (p. 45). Each part of speech is discussed first in language-general terms and then in language-specific terms.

Chapter four entitled 'The structure of kernel clauses' deals with 'two layers of structure: the VP and the clause' (p. 49).

Chapter five is about 'Tense, aspect and modality' (p. 69). Huddleston introduces the chapter:

> This chapter will be primarily concerned with the semantics of the tense inflections and of certain aspectual and modal catenatives; in addition we will consider, in the light of this semantic discussion and of what has already been said about the grammar, the nature of tense, aspect and mood/modality as general linguistic categories (p. 69).

Chapter six deals with 'Nouns and noun phrases'. Huddleston deals with the subject in an ordinary way, discussing determiners, number, countability, definiteness (including generic/nongeneric categories), modifiers (both pre-head and post-head dependents, such as *[the author] of this novel*), proper nouns, pronouns, nominalization, and an excellent discussion on constituent structure in noun phrases.

Chapter seven discusses 'Adjectives, determinatives and numerals'. Huddleston points out that English adjectives 'generally occur very much more readily in the non-progressive construction than in the progressive' (p. 108). He illustrates: *Ed was tall* is acceptable while *Ed was being tall* is not acceptable.

Chapter eight addresses the topic 'Adverbs and prepositions'. Of particular note to me is Huddleston's treatment of 'peripheral dependent [adverbs] in clause structure' (p. 120-21). It is a fresh approach to categorizing adverbs.

Chapter nine, a fairly comprehensive review of 'Clause type', commends itself well to those interested in teaching ESL. This classically complex grammatical structure for foreign learners of English is treated with great skill here. Rich in examples which illustrate the concepts introduced, at the least it is an additional resource for material to teach about clauses. We particularly like Huddleston's distinction between form and meaning in his categorization of clause types. For example, the declarative *Passengers are requested to remain seated* is most naturally a request or directive in meaning (illocutionary force) (p. 129).

Chapter ten provides an inventory of the ways English expresses 'Negation'. Clausal versus subclausal negation, nonaffirmatives, semantic scope and marked focus are the subheadings in this chapter.

Again, for ESL teachers, chapter eleven is 'must' reading. Dealing with 'The subordination of clauses', it gives some great examples to help teach this very complex area of English.

Chapter twelve entitled 'Thematic systems of the clause' deals with the different packages into which the same 'propositional content' goes (p. 173). For example, the active *My father wrote the letter* and the passive *The letter was written by my father* are 'prototypically thematic variants' (p. 173).

Finally, chapter thirteen discusses coordination, both 'basic' and 'nonbasic'. Nonbasic coordination involves: (a) discontinuity, (b) bound ellipsis or (c) restructuring. Again, Huddleston's treatment is clear, peppered with numerous illustrations.

Although the book is tough sledding as bedside reading, it is an excellent tool for learning categories of grammar, for gaining an overview of linguistics, and for gaining familiarity with linguistic terminology—all of which Huddleston explains clearly in addition to giving helpful comparative and contrastive examples. ■

English verb classes and alternations: A preliminary investigation. By Beth Levin. 1993. Chicago: The University of Chicago Press. xviii, 348 pp. Cloth $45.00, Paper $18.95.

Review by Charles Peck
SIL—International Linguistics Consultant

This book grows out of the MIT Lexicon project which has been active for about a decade but which follows research and interest at other centers for two or three decades. Investigators have been studying questions such as:

> What alternations (=?transformations) can involve what verbs and what verbs participate in what alternations?
> What are the basic components of the meaning of a verb that allow it to participate in certain alternations and not others?
> Which alternations are diagnostic?
> Which alternations are sensitive to what components of meaning?

The investigators want to capture diagnostic generalities without getting into the idiosyncrasies of particular verbs, just as speakers of the language do.

The book is in two parts. The first part is called 'Alternations' and comprises chapters one through eight (eight groups of alternations). This part groups the verbs by what alternations they can and cannot tolerate. The second part of the book is 'Verb Classes' and comprises chapters nine through fifty-seven. Here the verbs are described by classes with notes on what their common meanings are and in what alternations they participate. Many chapters are divided into sections and subsections. Many sections begin with a paragraph of references.

For example, if we look at chapter two we find all the 'alternations involving arguments within the VP'. These include dative alternation, benefactive alternation, locative alternation, creation and transformation alternation, reciprocal alternation, etc.

In chapter nine we find 'verbs of putting', which include Put verbs, Funnel verbs, Pour verbs, Coil verbs, Spray/Load verbs, Fill verbs, Butter verbs and Pocket verbs. The 'Put' verbs include arrange, immerse, install, lodge, mount, place, position, put, set, situate, sling, stash, and stow. The 'Pocket' verbs include archive, bag, bank, beach, bed, bench, berth, billet, bin, bottle, box, cage, can, cellar, cloister, and many more.

The 'put' verbs require a location prepositional phrase, but not with prepositions 'to' or 'from'. They do not have a related intransitive use and some have related name-nouns (such as 'lodge' and 'position'). The 'pocket' verbs all have related nouns that name the location where things can be put. Most of the verbs, however, do not favor being collocated with their related nouns. One does not pocket a comb in one's pocket—the location phrase is redundant. One just pockets a comb. The class of 'pocket' verbs is large and open; new members can be created easily.

The book has a complete index of the verbs, so I decided to trace down how it treats the verb 'pass'—a good multi-meaning, poly-semantic verb. 'Pass' occurs in section 1.1.2.1 where it is a member of the 'give' verbs under verbs of change of possession which do not permit an intransitive inchoative alternation, such as 'break' does (Janet broke the cup/the cup broke). It also occurs in section 1.2.4 where it is a member of the 'marry' verbs that have an understood reciprocal object (John and I pass each other in the hallway every day/I pass John/John passes me). It occurs in 1.3 where it is a member of the 'send' verbs that do not have a conative alternation, as for example 'push' does (I pushed the table/I pushed on/against the table).

'Pass' occurs in section 1.4.2 where it is a member of the 'marry' verbs again that do not have a 'with' preposition drop alternation as 'battle' does (I battled with John/I battled John). In section 2.1, it is a member of the 'give' verbs and a member of the 'throw' verbs, both of which participate in dative alternation (I passed John the bread/I passed the bread to John; Tom passed the football to John/Tom passed John the football). In section 2.5.4, it is a member of the 'marry' verbs that do not have an alternating reciprocal alternation (intransitive, using a prepositional phrase) as 'agree' does (I agree with John/John agrees with me/John and I agree). In section 6.1, 'pass' is a member of the inherently directed motion verbs that can have a post-verbal alternation (A car passed/There passed a car). And in section 6.2, it is again a member of the inherently directed motion verbs that have a locative inversion alternation (A car passed on the street/On the street passed a car).

'Pass' occurs in section 11.1 where it is listed a member of the 'send' class of verbs that participate in the dative alternation but not the conative alternation, the causative alternation, the middle alternation, nor the coreferential interpretation of pronouns. In section 13.1 it is a member of the 'give' verbs which participate in the dative alternation but not in the fulfilling alternation nor the causative alternation. In section 17.1 it is a member of the 'throw' verbs which take a goal preposition phrase, participate in the dative alternation but do not participate in other alternations. And in section 36.2 it is member of the 'marry' verbs that participate in the understood reciprocal object alternation but not the other alternations. 'Pass', however, is not listed in section 51.1 which discusses the class of inherently directed motion verbs, probably because it does not participate in all the alternations that the other verbs do.

Even with the detail above, the meaning of 'pass an examination/a course/muster/some statistic' is not included, nor the meaning 'pass into oblivion', 'pass out', 'pass out of view/fashion', nor 'pass a motion/ legislation'. However, most of these meanings are idiosyncratic. No other verbs have parallel possibilities and not much would be gained by including them.

Multiply the detailed data on the verb 'pass' by two thousand, and you can get a feeling for the scope of this book. It is the product of a lot of work by many people.

The book treats three or four thousand verbs, but there are many others such as 'postpone', 'impose', or 'assault' (taken from the front page of

yesterday's newspaper) that are not in the book. However, of the few such verbs I have checked, most would fit into classes that Levin has already set up. They are easy to check—try assigning one to a class and see if it participates in all the alternations that the other members of the class do.

Levin hopes the information in this book would help in lexicon construction for human use or for machine use. For each verb the alternations could be briefly indicated and only the idiosyncratic information would need larger treatment. Presumably, the alternation details would be contained in a brief grammar sketch somewhere in the lexicon. The back cover mentions that the book might also be useful in the teaching of English. ■

CONFERENCE ON FUNCTIONAL APPROACHES TO GRAMMAR
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in conjunction with the
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Abstract deadline is past.

Recently received in Linguistics Coordinator's office
NEWS ON FUNCTIONAL GRAMMAR
Newsletter No. 13, February 1994.

This is the first issue since Dec. 1990. Items of interest are a list of recent book-length publications on Functional Grammar (FG), a list of Working Papers in FG, information about IFOTT, 'a recently established institute in Amsterdam which coordinates research into Functional Grammar and various other (functional) approaches to language and language use'; and FUNKNET (originator T. Givon).

To be added to the mailing list,
write to: editors: Lachlan Mackenzie and Peter van Baarle
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The following books are available for review by our readers. If you wish to do a book review for publication in *Notes on Linguistics*, contact the editor, and the book will be shipped to you along with instructions for submitting the review. When you submit a review, the book is yours to keep. Contact:

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Alexrod, Melissa. 1993. The semantics of time: Aspectual categorization in Koyukon Athabaskan. Lincoln, Nebraska: University of Nebraska Press. 199 pp. Cloth $40.00

Bates, Madeleine and Ralph M. Weischedel, eds. 1993. Challenging in natural language processing (Studies in natural language processing). Cambridge, MA: Cambridge University Press. 296 pp. Hardcover $49.95.

Botha, Rudolf P., et al., eds. 1992. Linguistics and the language professions. Stellenbosch papers in linguistics (SPIL No. 25). Stellenbosch, South Africa: Dept. of Linguistics, University of Stellenbosch. 242 pp.

Charney, Jean Ormsbee. 1994. Grammar of Comanche. Lincoln, Nebraska: University of Nebraska Press. 271 pp. Cloth $35.00

Fagan, Sarah M. 1992. The syntax and semantics of middle constructions: A study with special reference to German. Cambridge: Cambridge University Press. 300 pp. $59.95.

Kimball, Geoffrey D. with the assistance of Bel Abbey, Nora Abbey, Martha John, Ed John, and Ruth Poncho. 1991. Koasati grammar (Studies in the anthropology of North American Indians). Lincoln: University of Nebraska Press. 640 pp. $70.00

McCawley, James D. 1981. 2nd printing 1993 Everything that linguists have always wanted to know about logic* *but were ashamed to ask. Chicago: The University of Chicago Press. 523 pp. First Edition. $22.95.

McGarry, Richard G. 1994. The subtle slant: A caross-linguistic discourse analysis model for evaluating interethnic conflict in the press. Boone, NC: Parkway Publishers. 195 pp. Hard cover $35.00 U.S.; $45.00 outside the U.S.

Mesthrie, Rajend. 1992. English in language shift: The history, structure and sociolinguistics of South African Indian English. Cambridge: Cambridge University Press. 252 pp. Hard cover $64.95.

Ojeda, Almerindo. 1993. Linguistitic individuals. (CSLI Lecture Notes Number 31) 190 pp. Paper $17.95.

Parks, Douglas R. 1991. Traditional narratives of the Arikara Indians. (Studies in the Anthropology of North American Indians) Lincoln: University of Nebraska Press.
Vol. 1. Stories of Alfred Morsette: Interlinear linguistic texts. 684 pp.
Vol. 2. Stories of other narrators: Interlinear linguistic texts. 687-1344 pp. Set of 2, hardcover $125.00
Vol. 3. Stories of Alfred Morsette: English translations. 468 pp. $75.00
Vol. 4. Stories of other narrators: English translations. 471-920 pp. $40.00. Set of four volumes $200.00.

Reports from Uppsala University Linguistics (RUUL No. 26). 1994. Papers by Björk (in Swedish), Wikholm (in Swedish), Öhman, Pettersson, Saxena. Uppsala, Sweden: Dept. of Linguistics, Uppsala University. 111 pp.

Rosner, Michael and Roderick Johnson, eds. 1992. Computational linguistics and formal semantics. (Studies in natural language processing, Branimir K. Boguraev, ed.). Cambridge: Cambridge University Press. 321 pp. Cloth $74.95.

SALSA I. 1993. Proceedings of the First Annual Symposium about language and society - Austin. Editors: Robin Queen and Rusty Barrett. University of Texas, Dept. of Linguistics, Austin, Texas. (Volume 33, Texas Linguistic Forum).

Scharma, J. C., ed. 1992. From sound to discourse: A tagmemic approach to a Indian languages. xxiv, 313 pp. (Contents include two articles by Kenneth L. Pike: 1 Recent developments in tagmemics; 2 An autobiographical note on my experience with tone languages.) Manasagangotri, Mysore: Centeral Institute of Indian Languages. Paper $5.00.

Troelstra, A. S. 1992. Lecture notes: Lectures on linear logic. (CSLI Lecture Notes No. 29) Stanford, CA: Center for the Study of Language and Information, Leland Stanford Junior University. 200 pp. Hardcover $49.95; Paper $18.95. ■

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From the Linguistics Coordinator

One of the primary objectives of the SIL Linguistics Coordinator's office is to disseminate among field teams and consultants various helps on doing linguistics and materials for keeping up-to-date in the field. *Notes on Linguistics* is one forum for doing this.

An increasingly more important forum of helps for field teams is SIL linguistics software and manuals for doing field linguistics on computer. By far the current major time expenditure of the staff who relate to the Linguistics Department involves developing, monitoring and reviewing manuals related to linguistics software being developed in SIL.

In coordination with the Academic Computing Department and the Project '95 Administration (both being departments of the Academic Affairs Section of SIL), for several years a team lead by Bruce Hooley and Peter Wang, with a major contribution from Les Bruce, has been working on an on-line Linguistics Field Manual, under what has recently been named 'LinguaLinks', and due for first release in the summer of 1995. A preliminary version is currently being implemented with Field Methods students at the Texas SIL School. The most highly developed aspect of the first release is a lexical database, which is an aid to doing dictionary work, semantic research and interlinear text glossing. Integrated with this is also an aid for doing beginning phonology analysis.

In coordination with the International Computer Services Department of JAARS, the Linguistics Department has been involved in reviewing manuals accompanying two pieces of software being published by SIL: *FindPhone: Phonological analysis for the field linguist* by David Bevan, and *Making Dictionaries: A guide to lexicography and the Multi-Dictionary Formatter* By David F. Coward and Charles E. Grimes. Both of these have been 'previewed' in previous *Notes on Linguistics* articles. (See 57.4-10 and 61.28-39 for *FindPhone*, and see 66.5-25 for *Multi-Dictionary Formatter).*

Finally, another major focus of our department in this arena is the CADA Guidance Team (CADA = Computer Assisted Dialect Adaptation). Andy Black chairs the meetings of this team and generally coordinates the Linguistics Department's role in matters relating to CADA. We welcome Andy as a new International Linguistics Consultant with specific responsibilities in CADA coordination and consulting for our department..

—David Payne

CONGRATULATIONS to the following SIL members recently completing PhD's in Linguistics:

Cheri Black (Mexico Branch, formerly Peru Branch), University of California at Santa Cruz
Michael Boutin (Malaysia Branch), University of Florida
Sherri Brainard (Philippines Branch), University of Oregon
Constance Kutsch Lojenga (Eastern Zaire Group), University of Leiden

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ERRATA

The following correction should be made on the front page of *Notes on Linguistics No. 66*: The title of the review by Mike Cahill should read: *The phonology of tone, Harry van der Hulst and Keith Snider, Eds.* We apologize for this typographical error.

The category 'Irrealis' in Papuan medial verbs

John R. Roberts
SIL—Papua New Guinea Branch

1. Introduction. In Roberts (1990) I presented data from a number of Papuan languages as evidence that in these languages there was a morphological distinction marked on the medial switch-reference (SR) verb form that could be analyzed as a simple binary distinction of 'realis' versus 'irrealis' modality, where realis relates to events that the speaker believes to be actualized or realized, and irrealis relates to events that the speaker believes have not been actualized. The term 'irrealis' has been used by a number of linguists to refer to a general modal category with this meaning, e.g. Givón (1984, 1992), Chung and Timberlake (1985), and Foley (1986).

The view that the category irrealis as presented in Roberts (1990) can be understood as a coherent if not homogenous category has been challenged by subsequent writings, most notably Bugenhagen (1991) and Bybee, Pagliuca and Perkins (BPP) (*in press*). This article[1] is therefore a response to these challenges.

Bugenhagen examined the semantics of 'irrealis' forms as described for fourteen Austronesian languages. He traces the use of the term irrealis in Austronesian linguistics back to Dempwolff (1939) via Capell (1971). In fact, Capell (1971:288) considered the category irrealis a striking feature of certain Austronesian languages, i.e. those with SOV basic word order and postpositions:

> A further feature of AN_2 languages worthy of attention is the general presence of a realis-irrealis distinction in the verbal systems, i.e. a basic distinction between actions which are regarded as actually occurring and actions which are merely thought about.

---

[1] This article is a revised version of a paper presented at the Symposium on Mood and Modality held at the University of New Mexico, Albuquerque in May 1992. It is primarily a response to the challenges raised against the arguments for an irrealis category in Papuan medial verbs as presented in Roberts (1990). I have endeavoured to take into account the comments received on the paper presented at the symposium and the usual disclaimers apply.

In his survey, however, Bugenhagen found that the contexts in which irrealis forms were used in the Austronesian languages investigated were, in his terms, 'extremely heterogenous'. As well as including many contexts where irrealis would be expected, such as future tense, hypothetical conditional, purpose, 'want' verbs, 'lest' verbs, ability, obligation, counterfactual, imperative, prohibitive, doubt, negation and certain future, putative irrealis forms in several languages were used in contexts where they would not be expected, i.e. habitual and even present time. The occurrence of irrealis forms in habitual contexts occurred in the Manam and Sinaugoro languages and the occurrence of irrealis forms in a present time context occurred in the Tigak language. In Tigak there are two different sets of subject pronouns, which Bugenhagen (1991:33) says indicate the realis-irrealis distinction but he terms as 'past' versus 'non-past' forms. Past pronouns differ from non-past pronouns by having a suffix final *-a*. Non-past pronouns occur in a present time context as well as future time and modal contexts. In conclusion, however, Bugenhagen observed that there did appear to be a core or prototypical set of contexts in which such irrealis forms were found, viz. (1) future, (2) hypothetical conditionals, (3) purpose clauses, (4) complements of predicates expressing 'want' and (5) negative purpose clauses. Bugenhagen suggested that the semantic invariant of this prototypical domain of contexts can be specified as:

[... irrealis form...][1]

I think:
There is no time when this (=S) happened/was[2]

In fact, in all of the languages in Bugenhagen's survey an irrealis form was obligatory for future tense. So the context of unmodulated future time reference would seem to be the prototypical context of irrealis forms in these Austronesian languages. It would also seem to be the case that the context of unmodulated past time reference is the prototypical context of realis forms in these Austronesian languages.

BPP (63) also challenge the validity of an overarching irrealis category:

> For instance, Roberts (1990:399) confronts the problem of Bargam (a Papuan language) which treats the past habitual as irrealis. Roberts suggests that the

---

[2] Bugenhagen slightly changed his definition of irrealis in a subsequent revision of his paper for publication by excluding the *there is* component.

> interpretation of the notion 'real world' differs across languages. We suggest that if this binary distinction differs so much across languages that a past tense, which is usually considered one of the prototypical cases of realis (Foley 1986:158f), can be irrealis in some languages, this binary distinction is not cross-linguistically valid.

BPP (62) also say that the realis-irrealis distinction is rarely realized in a language as a simple binary morphological distinction and that none of the seventy-six languages in their GRAMCATS survey exhibited such a marking.

As an initial response to this last objection by BPP that few, if any, languages mark a binary realis-irrealis distinction morphologically I would cite first of all the five Papuan languages described in Roberts (1990), viz. Amele, Nobonob, Anjam, Wojokeso, and Bargam, in which a binary realis-irrealis distinction is marked on the medial verb form. I would also cite the Manam, Muyuw, Patep, Sursurunga and Tigak Austronesian languages in Bugenhagen's survey in which a binary realis-irrealis distinction is marked, i.e. there is just one form that is associated with an irrealis meaning. Ross (1987) also describes the Austronesian languages, Takia, Gedaged, Bilibil and Matukar of the Belan sub-family in which a binary realis-irrealis distinction is marked morphologically on the medial dependent verb forms. I would also further cite the following Papuan languages that are described as having a binary realis-irrealis distinction marked in the verb morphology: Salt-Yui (Irwin, 1974), Menya (Whitehead, 1991), and Bukiyip (Conrad and Wogiga, 1989). In Salt-Yui the only 'tense' distinction marked on the verb is between realis, which is zero marked, and irrealis, marked by < *-na* >. The irrealis form occurs in future time and hypothetical contexts. In Menya irrealis modality is indicated by a distinctive set of subject person-number agreement markers. In Bukiyip only one tense-mood distinction is marked on the verb and that is between realis, marked with *a-*, and irrealis, marked with *u-*. Realis mood is used to express all events that occurred in past and present time and irrealis mood is used to express all future time events and all events that did not actually happen in the past. In Bukiyip the irrealis verb form is also used to express background information in oral narrative text.

It is also the case that a binary realis-irrealis marking is not just restricted to Papuan and Austronesian languages found in and around Papua New Guinea (PNG). In Austronesian languages in general the realis-irrealis division is the major distinction marked in the verbal inflection instead of a tense distinction, e.g. Fijian (Dixon, 1987). A binary realis-irrealis

distinction marked on the SR medial verb is also found in North American Indian languages, e.g. Central Pomo (Mithun, 1992). In Central Pomo the cognitive basis of irrealis is hypothetical, since realis as well as irrealis medial forms can co-occur with final future forms. The occurrence of a realis-future combination indicates the speaker's evaluation that the future event will definitely happen. Chafe also (1992) describes the realis-irrealis pronominal prefixes that are an obligatory feature of the verb in the Caddo language. Contexts in which the irrealis prefixes occur are: yes-no questions, negation, prohibitions, obligations, conditionals, and with a number of prefixes expressing the more unusual categories of simulative, 'as if', infrequentative, 'seldom', and the admirative which expresses surprise. Contexts in which the realis prefixes occur include: question-word questions, futures and imperatives. So the semantics of the Caddo realis-irrealis distinction is quite different in content to that found in Austronesian and Papuan languages and Central Pomo. Nevertheless, a category termed 'irrealis' with a core semantic context of future-hypothetical does appear to exist in Austronesian, Papuan and North American Indian languages. BPP would therefore have to ignore large language groupings in the Pacific and North America in order to maintain their claim that a binary realis-irrealis distinction does not exist in languages of the world.

In this article I will endeavour to show that the category irrealis in Papuan medial verbs as described in Roberts (1990) is not arbitrary nor whimsical as BPP maintain but can be validated as a prototypical category[3] that has a core meaning based on future tense. I will also suggest a possible explanation for the origin and development of such a category through the process of tense neutralization in Papuan medial verbs.

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[3] A prototype category is defined in terms of 'core' or prototypical members of the category, which function as exemplars of the category. Prototype categories do not have clear boundaries and one category can merge into another. In fact, Rosch (1978) has shown that human beings categorize the world around them on a prototypical basis and not on a 'classical' basis where categories are defined in terms of a conjunction of necessary and sufficient conditions and have invariable boundaries. So all categories including linguistic categories are formed on a prototypical basis. Givón (1984), Lakoff (1987), Langacker (1987) and Taylor (1989) have also demonstrated how the prototype approach to linguistic analysis can be extended to all linguistic levels including lexical semantics, morphology, syntax and phonology.

2. A recapitulation of Irrealis in Papuan medial verbs. As mentioned in Roberts (1990) many Papuan languages have a characteristic feature of dependent or medial clauses that contain a verb marked for SR, that is, they indicate if the subject of the following verb is the same as (SS) or different from (DS) the subject of the marked verb. One such language is Amele.[4] In Amele a distinction is marked on the medial verb of SS versus DS.[5] With each of these categories a distinction is also marked between sequential (SEQ) versus simultaneous (SIM) tense. Within the SIM category a distinction is also marked between durative (DUR) versus punctual (PUNC) aspect. The morphology of the Amele medial verb is illustrated in Table 1. SEQ.SS is marked by the morpheme *-me* followed by subject agreement (SuAgr) set 1. SEQ.DS is marked by the morpheme *-cV* (*c* is [ʔ] and V is a harmonic vowel) followed by SuAgr set 2. SIM.SS is marked by SuAgr set 1. SIM.SS.DUR is marked by reduplication of some part of the verb, usually the first CV,[6] otherwise, if the category is SIM.SS.PUNC, then there is no reduplication of the verb.

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[4] Amele belongs to the Gum language family (see Z'graggen, 1975) and is spoken by about 6000 people living just south of the town of Madang in Papua New Guinea. The grammar of Amele is extensively described in Roberts (1987) and the switch-reference system is also discussed in Roberts (1988a, b). The following abbreviations are used in this article: 1(st person), 2(nd person), 3(rd person), INC(lusive person), EXC(lusive person), S(ingular), D(ual), P(lural), O(bject), S(ame)S(ubject), D(ifferent)S(ubject), REM(ote)P(ast tense), TOD(ay's)P(ast tense), PRES(ent tense), FUT(ure tense), HAB(itual)P(ast), IMP(erative mood), HO(rtative mood), PROH(ibitive mood), CO(u)NT(e)R(factual mood), IND(icative mood), DES(iderative), DUR(ative aspect), PUNC(tual aspect), PROG(ressive aspect), R(ealis), IR(realis), SEQ(uential tense), SIM(ultaneous tense), NEG(ative), NEG(ative)F(uture tense), NEG(ative)P(ast tense), INF(initive), BEN(efactive), ERG(ative), CONJ(unction), Su(bject)Agr(eement), Antic(ipatory Subject)Agr(eement).

[5] In fact, the SS and DS morphology in Amele only prototypically indicates same or different subject following. As discussed in Roberts (1988b), this morphology can also indicate changes in time, place and world setting.

[6] For a full discussion of reduplication in the Amele verb as well as other forms of reduplication in Amele see Roberts (1991).

Table 1. Amele switch-reference system.

verb stem +								
SS:	SEQ: -me	+		S	D	P		
			1	-iq	-u=ø	-b	SuAgr	
			2	-q	-si	-iq	set 1	
			3	-i	-si	-iq		
	SIM: ± redup. stem							
DS:				S	D	P		
	SEQ: -cV	+	1	-min	-hul	-mun	SuAgr	
			2	-m	-bil	-bil	set 2	
			3	-b	-bil	-bil		
	SIM.IR: ± redup. stem							
				S	D	P		
	SIM.R: ± redup. stem	+	1	-iqin	-wan	-qon	SuAgr	
			2	-gan	-sin	-qin	set 3	
			3	-n	-sin	-qin		

Amele also has a range of categories marked on the final verb, i.e. the verb that is in the clause at the end of the SR clause chain, which are quite different to the categories marked on the medial verb. These include present tense marked by *-na*, today's past tense marked by *-a*, yesterday's past tense marked by *-an*, remote past tense marked by portmaneau SuAgr markers, past habitual aspect marked by *-lo*, future tense marked by *-e* ≈ *-an* and particular SuAgr markers, relative future tense marked by *-e* ≈ *-a* and particular SuAgr markers, counterfactual mood marked by *-u*, negative past tense marked by the particle *qee* 'not' and the verb suffix *-l*, negative future tense marked by the particle *qee* 'not' and the infix <*u*> within the future tense forms, and imperative mood marked by *-a*. The final verbs are not marked for the categories found on the medial verbs, viz. SS versus DS, SEQ versus SIM, and DUR versus PUNC. However, the tense, mood, and aspect categories marked on the final verb, while not marked as such on the medial verb, do apply to the medial verb forms. The medial verbs are therefore within the scope of the final verb in the clause chain with respect to tense, mood, and aspect categories. So in examples (1-4), for instance, the medial SR verb is understood to have the same tense as the final verb. This being yesterday's past in (1), future in (2), present in (3), and remote past in (4) (*q* is [gb]). However, these tense categories are not marked overtly on the medial forms in these cases.

(1) *Age cabi na beli-me-ig ceta ceh-eig-an.*
3P garden to go-SS-3P yams plant-3P-YESTP
They went to the garden and planted yams (yesterday). (Amele)

(2) *Age cabi na beli-me-ig ceta ceh-oqag-an.*
3P garden to go-SS-3P yams plant-3P-FUT
They will go to the garden and plant yams.

(3) *Ege cabi na be-bel-oqon ceta ceh-egi-na.*
1P garden to DUR-go-DS.1P.SIM yams plant-3P-PRES
While we go to the garden they are planting yams.

(4) *Ege cabi na cal-eb ceta ceh-om.*
1P garden to arrive-SS.1P.SIM.PUNC yams plant-1P.REMP
When we arrived at the garden we planted yams.

There is, in fact, one medial verb form in Amele where tense-mood concord is marked overtly with the final verb. This is in the case of the DS.SIM medial verb as illustrated in Table 1 but not discussed as yet. For the DS.SIM medial verb there are two sets of SuAgr (sets 2 and 3). The verb final categories that co-occur with the set 3 SuAgr marked medial verb include all the past tenses (today's past, yesterday's past, and remote past), past habitual aspect, and present tense. The verb final categories that co-occur with the set 2 SuAgr marked medial verb include the future tenses and imperative, prohibitive, counterfactual, prescriptive, hortative, and apprehensive moods. This simple binary marking on the DS.SIM verb divides the final verb categories into two basic semantic domains, which I have termed 'realis' for set 3 SuAgr and 'irrealis' for set 2 SuAgr. Realis defines events that the speaker thinks have happened and therefore are categorized as 'real' and irrealis defines events that the speaker thinks have not happened and therefore are categorized as 'unreal' in some way. Sample sentences with realis forms are given in (5) and sample sentences with irrealis forms are given in (6).

(5) a. Ho *bu-busal-en age qo-in.* (REALIS)
pig DUR-run out-3S.DS.SIM.R 3P hit-3P.REMP
They killed the pig as it ran out.

b. *Ho bu-busal-en age qo-ig-a.*
pig DUR-run out-3S.DS.SIM.R 3P hit-3P-TODP
They killed the pig as it ran out.

c. *Ho bu-busal-en age qo-igi-na.*
pig DUR-run out-3S.DS.SIM.R 3P hit-3P-PRES
They are killing the pig as it runs out.

d. *Ho bu-busal-en age qo-lo-ig.*
pig DUR-run out-3S.DS.SIM.R 3P hit-HABP-3P
They used to kill the pig as it ran out.

(6) a. *Ho bu-busal-eb age qo-qag-an.* (IRREALIS)
pig DUR-run out-3S.DS.SIM.IR 3P hit-3P-FUT
They will kill the pig as it runs out.

b. *Ho bu-busal-eb age qo-ig-a.*
pig DUR-run out-3S.DS.SIM.IR 3P hit-3P-IMP
Kill the pig as it runs out.

c. *Ho bu-busal-eb ege q-oc nu.*
pig DUR-run out-3S.DS.SIM.IR 1P hit-INF HO
Let us kill the pig as it runs out.

d. *Ho bu-busal-eb age qo-u-b.*
pig DUR-run out-3S.DS.SIM.IR 3P hit-CONTR-3P
They would / should have killed the pig as it ran out.

e. *Ho bu-busal-eb cain qo-wain.*
pig DUR-run out-3S.DS.SIM.IR PROH hit-NEGF.3P
Don't kill the pig as it runs out.

f. *Ho bu-busal-eb age qo-wain (dain).*
pig DUR-run out-3S.DS.SIM.IR 3P hit-NEGF.3P lest
Lest they kill the pig as it runs out.

Note that in the irrealis domain there are semantic modal categories that would be classified as different basic types of modality within a Western philosophical framework, such as (6b) imperative = deontic modality or (6d) counterfactual = epistemic modality.

It should be noted that the medial DS.SIM verb is in a coordinate relationship[7] with the final verb so it therefore concords with the final verb in modal status. It is ungrammatical, for example, to have an irrealis medial verb with a realis final verb and vice versa, as in (7).

(7) a. *Ho *bu-busal-eb age qo-igi-na.*
pig DUR-run out-3S.DS.SIM.IR 3P hit-3P-PRES

b. *Ho *bu-busal-en age qo-qag-an.*
pig DUR-run out-3S.DS.SIM.R 3P hit-3P-FUT

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[7] For a full discussion of the arguments that SS/DS medial clauses are in a coordinate relationship to final clauses in Amele see Roberts (1988a).

Notwithstanding the fact that the distinction between coordination and subordination has been demonstrated by a number of linguists to be a cline from one to the other rather than a disjunctive opposition of two poles[8] there are several syntactic criteria that can be used to distinguish coordinate medial clauses from subordinate medial clauses in most Papuan languages. These criteria are:

(i) A subordinate medial clause can be structurally embedded within another matrix clause and can be in a dependency relationship with the verb of the matrix clause. A coordinate medial clause cannot be so embedded.

(ii) A subordinate medial clause can be extraposed to the end of the matrix clause, whereas a coordinate medial clause is usually fixed sequentially and cannot be extraposed.

These distinctions can be demonstrated from Amele. The first clause in (8a) is an adverbial clause expressing purpose. The verb *qoqagan* is marked for the final verb tense category of future tense. Amele has a fairly free word order with respect to adverbial elements so that it is possible for the orders AdvCl S V and S AdvCl V to occur as in (8). The second order, S AdvCl V (8b), is in fact the order preferred by native speakers. So (8b) is preferred to (8a).

(8) a. *Ho* *qo-qag-an* *nu* *dana age* *h-oig-a.*
pig hit-3P-FUT PURPOSE man 3P come-3P-TODP
The men came to kill the pig.

b. *Dana age* ***ho qoqagannu*** *hoiga.*
The men came to kill the pig.

However, a clause coordinated with the conjunction *iqa* 'but' cannot be embedded within another clause, as illustrated by (9) below.

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[8] For example, Huddleston (1984:378-418) and Quirk et al. (1985:927) discuss the gradience between coordination and subordination in English and Foley and Van Valin (1984) and Haiman and Thompson (1988) discuss the gradience between coordinate and subordinate structures in languages in general.

(9) a. *Ho busale-i-a qa dana age qo-ig-a.*
pig run out-3S-TODP but man 3P hit-3P-TODP
The pig ran out but the men killed it.

b. **Dana age* ***ho busaleia qa*** *qo-ig-a.*

With respect to embedding, a medial SR verb cannot be embedded within another clause. The verb in the first clause in (10a) is a medial SR verb marked for DS. However, it is not possible to move the first clause in (10a) between subject and verb in a similar way to (8b). (10b) is not acceptable.

(10) a. *Ho busale-ce-b dana age q-oiga.*
pig run out-DS-3S man 3P hit-3P-TODP
The pig ran out and the men killed it.

b. **Dana age* ***ho busaleceb*** *qoiga.*

It is usually possible to extrapose a subordinate clause and end-shift it to the end of the sentence for purposes of focus expression. So the purpose clause in (11a) can be shifted to the end of the sentence as in (11b) and the protasis in (12a) can be shifted to the end of the conditional sentence as in (12b).

(11) a. *Dana age ho qo-qag-an nu h-oig-a.*
man 3P pig hit-3P-FUT PURPOSE come-3P-TODP
The men came to kill the pig.

b. *Dana age* ___ *hoiga* ***ho oqagannu.***
The men came to kill the pig.

(12) a. *Ija ja hud-ig-en fi uqa sab man-igi-an.*
1S fire open-1S-FUT if she food roast-3S-FUT
If I light the fire she will cook the food.

b. ___ *Uqa sab manigian* ***ija ja hudigenfi.***
She will cook the food if I light the fire.

However, a coordinate clause cannot be end-shifted. For example, the first clause in (13a) conjoined with *qa* 'but' cannot be end-shifted as in (13b) and the first clause in (14a) conjoined with *fo* 'or' cannot be end-shifted as in (14b).

(13) a. *Ija ja hud-ig-a qa uqa sab mane-i-a.*
1S fire open-1S-TODP but 3S food roast-3S-TODP
I lit the fire but she cooked the food.

b. *___ *Uqa sab maneia* ***ija ja hudigaqa.***

(14) a. *Uqa ja hud-igi-an fo qee sab man-igi-an.*
3S fire open-3S-FUT or not food roast-3S-FUT
She will light the fire or cook the food.

b. *___ *Uqa sab manigian* ***ija hudigianfo qee.***

It is also usually not possible to extrapose an SR medial clause. For example, the first clause in (15a), which has a DS medial verb, cannot be end-shifted as in (15b).

(15) a. *Ho busale-ce-b dana age q-oiga.*
pig run out-DS-3S man 3P hit-3P-TODP
The pig ran out and the men killed it.

b. *___ *Dana age qoiga* ***ho busaleceb.***

However, if an SR medial clause is marked as subordinate then it can be extraposed. For example, in (16a) the DS medial clause is subordinated by the conjunction *nu* 'purpose' so it is possible for it to be extraposed as in (16b). In (17a) the DS medial clause is subordinated by the conjunction *fi* 'if' so it is also possible for this clause to be extraposed as in (17b).

(16) a. *Dana age ho qo-co-bil nu h-oig-a.*
man 3P pig hit-DS-3P PURPOSE come-3P-TODP
The men came to kill the pig.

b. *Dana age ___ hoiga,* ***ho qocobilnu.***
The men came to kill the pig.

(17) a. *Ho busale-ce-b fi dana age qo-qag-an.*
pig run out-DS-3S if man 3P hit-3P-FUT
If the pig runs out the men will kill it.

b. *___ Dana age qoqagan* ***ho busalecebfi.***
The men will kill the pig if it runs out.

It is also possible in Amele to coordinate subordinate clauses with a coordinating conjunction such as *ca* 'and', as in (18) for example.

(18) *Dana age ho qo-qag-an nu ca*
man 3P pig hit-3P-FUT PURPOSE and

gel ceh-oqag-an nu ca h-oig-a.
fence plant-3P-FUT PURPOSE and come-3P-TODP
The men came to kill the pig and to build the fence.

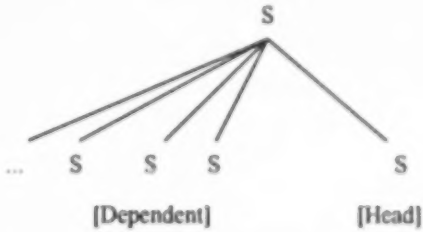
However, it is not possible to coordinate SR medial clauses with *ca* 'and', as in (19) for example.

(19) *Dana age ho q-u-me-ig (*ca) gel haun ceh-i-me-ig*
man 3P pig hit-PRED-SS-3P and fence again plant-PRED-SS-3P

*(*ca) jobon cesel-i bel-eig-a.*
and village return-PRED go-3P-TODP
The men killed the pig, rebuilt the fence and went home.

These syntactic criteria therefore show that SR clause chains in Amele, and in other PNG languages too, are endocentric constructions comprising a final clause as head and SR medial clauses functioning as dependent elements. This structure is illustrated by (20).

(20)



They are the clausal equivalent of headed phrases, such as NP or VP. In a headed phrase constituent the head can occur as the sole exponent of the constituent. For example, a noun or a verb can occur as the sole exponent of NP or VP respectively. In the same way S can occur as the sole exponent of a clause chain. Also in a headed phrase the head and its dependent elements are on the same level structurally but the head determines any relationships of concord or government over or within the phrase. In the English NP, for example, number is determined by the head noun but can be marked over the whole phrase, as in *this boy* (singular number) versus *these boys* (plural number). By the same token in a clause chain construction like (20) verbal categories such as tense, mood and negation are normally marked in full on the final clause, which functions as the head

of the chain, and then there is concord or agreement between the head clause and the preceding dependent clauses for these categories.

On the basis of the foregoing, the realis versus irrealis distinction marked on the DS.SIM medial verb in concord with the tense and modal categories marked on the final verb would appear to be just as valid as the other medial verb binary categories of SS versus DS, SEQ versus SIM and DUR versus PUNC.

It was also demonstrated in Roberts (1990) that negation stands outside of the realis-irrealis modal system in Amele. There are three past tenses: today's past, yesterday's past, and remote past (referring to events that have occurred prior to yesterday), but these distinctions are neutralized in the negative past and there is only one negative past tense form. Other categories, such as past habitual aspect, present and future tense, and imperative and counterfactual mood, have their own negative forms. However, because verbal negation operates across all tense, mood, and aspect systems in Amele it does not interact with the DS.SIM irrealis system. Therefore negated realis final forms do not trigger irrealis DS.SIM forms, as illustrated by (21).

(21) a. *Ho bu-busal-en/*-eb age qee qo-l-oin.*
pig DUR-run out-3S.DS.SIM.R/IR 3P not hit-NEGP-3P
They did not kill the pig as it ran out.

b. *Ho bu-busal-en/*-eb age qee qo-lo-ig.*
pig DUR-run out-3S.DS.SIM.R/IR 3P not hit-HABP-3P
They did not used to kill the pig as it ran out.

c. *Ho bu-busal-en/*-eb age qee qo-gi-na.*
pig DUR-run out-3S.DS.SIM.R/IR 3P not hit-3P-PRES
They are not killing the pig as it runs out.

The fact that negation does not interact with the realis-irrealis system would indicate that the meaning of this distinction is not just as to whether an event has happened or not, since verbal negation applies equally to the realis and irrealis forms. Rather the distinction is that the speaker asserts the truth or falsity of a proposition (realis-assertion) or does not assert the truth or falsity of a proposition (irrealis-nonassertion). Therefore in these languages the realis-irrealis-polarity configuration is one of:

I believe it is the fact that X	(REALIS-POSITIVE)
I believe it is the fact that not X	(REALIS-NEGATIVE)
I do not believe it is the fact that X	(IRREALIS-POSITIVE)
I do not believe it is the fact that not X	(IRREALIS-NEGATIVE)

For this reason it seemed necessary in Roberts (1990) to invoke the notion of 'real world' versus 'unreal world(s)' to account for the domain of meaning indicated by the realis-irrealis distinction marked on the DS.SIM medial verb. Realis refers to events that the speaker believes are true or false in the real world. Within the irrealis domain there are three basic types of events to refer: (1) events that are potentially true or false in the real world, e.g. futures, (2) events that are not true or false in the real world, e.g. counterfactuals, and (3) events that used to be true or false in the real world, e.g. past habituals.

Amele also has a set of sentence-final particles which can express modal notions such as assertion, contra-expectation, exhortation, permission granted, supplication, 'lest' and so forth. As explained in Roberts (1990) some of these particles can have scope over the whole sentence, i.e. the whole proposition, or just have scope over one item in the sentence. However, like the verbal negation, they do not interact with the medial DS.SIM verb irrealis marking. For example, in (22a) the addition of the particle *da*, which expresses contra-expectation, does not trigger an irrealis form on the medial DS.SIM verb nor in (22b) does the addition of the particle *fa*, which expresses doubt or doubtful question, trigger an irrealis form on the medial DS.SIM verb.

(22) a. *Ho bu-busal-en/*-eb age qo-in da.*
pig DUR-run out-3S.DS.SIM.R/IR 3P hit-3P.REMP nevertheless
Nevertheless, they did kill the pig as it ran out.

b. *Ho bu-busal-en/*-eb age qo-igi-na fa.*
pig DUR-run out-3S.DS.SIM.R/IR 3P hit-3P-PRES maybe
Maybe they are killing the pig as it runs out.

The sentence particles, while being a system for expressing modal notions in Amele, stand outside of the medial verb system just as verbal negation does. Indeed, there are a number of other ways of expressing modal notions in Amele with periphrastic verbal forms. In (23a) necessary obligation is expressed with an adverbial infinitive and the adverb *bahic* 'very, really, must'. In (23b) uncertainty is expressed with an adverbial infinitive and the conjunction *qa* 'but, if'. In (23c) intention is expressed with an adverbial infinitive and the postposition *nu* 'for'. In (23d) ability is expressed with an adverbial infinitive and the postposition *we* 'able'. In (23e) desiderative is expressed by an impersonal verb construction with an embedded imperative. In (23f) unfulfilled desire is expressed by an impersonal verb in the counterfactual mood with an embedded past tense verb.

(23) a. *Ija nu-ec bahic nu-ig-en.*
1S go-INF must go-1S-FUT
I must go.

b. *Ija nu-ec qa nu-ig-en.*
1S go-INF but go-1S-FUT
I might go.

c. *Ija nu-ec nu nu-ig-en.*
1S go-INF PURPOSE go-1S-FUT
I am going to go.

d. *Ija nu-ec we nu-ig-en.*
1S go-INF able go-1S-FUT
I am able to go.

e. *Ija nu-ug-a te-ß-na.*
1S go-2S-IMP 1S.O-3S-PRES
I want to go.

f. *Ija nu-em to-u-b.*
1S go-1S.REMP 1S.O-CONTR-3S
I would like to have gone.

None of these forms, however, interact with the medial verb modal system, since this system is restricted to interacting with just the tense, mood, and aspect categories marked on the final verb. It would also seem to be the case that the notion of asserted versus non-asserted belief in the truth or falsity of the proposition does not fit well in the Amele case since assertion itself is expressed by the sentence particle *ijom*, as in (24). However, this could be understood as emphatic assertion.

(24) *Ho bu-busal-en age qo-in ijom.*
pig DUR-run out-3S.DS.SIM.R 3P hit-3P.REMP ASSERTION
They really did kill the pig as it ran out.

The medial verb modal system is, in fact, the most grammaticalized of the modal systems in the Amele language and therefore would be expected to form the most coherent semantic domain of the various modal systems. From the language internal evidence there would appear to be every justification for the categorial distinction of realis versus irrealis on the Amele DS.SIM medial verb which divides the tense, mood, and aspect categories of the final verb forms into two basic semantic domains as illustrated in Table 2.

Table 2. The modal status of Amele final verb categories

REALIS	IRREALIS
present tense	future tense
today's past tense	relative future tense
yesterday's past tense	imperative mood
remote past tense	hortative mood (infinitive form)
past habitual aspect	counterfactual mood
	prescriptive mood
	apprehensive mood
	purpose (infinitive form)
negative present tense	negative future tense
negative past tense	negative relative future tense
negative past habitual aspect	prohibitive mood
	negative hortative mood (infinitive form)
	negative counterfactual mood
	negative prescriptive mood
	negative apprehensive mood
	negative purpose (infinitive form)

In order to substantiate the realis versus irrealis analysis of the DS.SIM medial verb forms in Amele, I presented in Roberts (1990) data from a number of other Papuan languages to show that in these languages essentially the same realis versus irrealis distinction is marked on the medial verb. The languages cited were Nobonob (Aeschliman, 1988), Anjam (Rucker, 1983), Wojokeso (West, 1973), and Bargam (Hepner, 1986). Nobonob, Anjam and Wojokeso have systems very similar to Amele in that different sets of SuAgr markers on the SR medial verb serve to divide the verb final categories into realis and irrealis domains. In Nobonob the realis categories are: present and past tense and past habitual, and the irrealis categories are: future tense, and imperative and counterfactual mood. Nobonob also has a set of sentence particles that do not interact with the irrealis medial verb forms. In Nobonob, too, verbal negation is outside of the medial verb modal system. In Anjam the realis categories are: present, immediate past (up to yesterday), and remote past tense. The irrealis categories are: future tense, and imperative and counterfactual mood. In Anjam verbal negation is also outside of the medial verb modal system. In Wojokeso the realis categories are: present, narrative past, near past and remote past tense, and past habitual. The irrealis categories are: near future tense, subjunctive, counterfactual and imperative mood. Bargam, however, is somewhat different from the other languages cited in that in Bargam realis and irrealis are distinguished by invariable morphemes rather than by different sets of SuAgr. Also in Bargam the

distribution of verb final categories in the realis and irrealis domains is somewhat different to the distribution in the other languages. In Bargam the realis categories are present and past tense; the irrealis categories are future tense, and imperative, hortative and counterfactual mood—also past habitual. This distribution of final verb realis and irrealis categories is summarized in Table 3.

Table 3. Realis-irrealis final verb categories marked by medial verb inflection

	Amele	Nobonob	Anjam	Wojokeso	Bargam
R	hab.p	hab.p		hab.p	
E	rem.p		rem.p	rem.p	
A	yest.p			narr.p	
L	tod.p	past	imm.p	near.p	past
I	pres.	pres.	pres.	pres.	pres.
S	including negative categories				
I	fut.	fut.	fut.	fut.	fut.
R	imp.	imp.	imp.	imp.	imp.
R	hort.			hort.	hort.
E	contr.	contr.	contr.	contr.	contr.
A	app.			subj.	
L					hab.p
I					
S	including negative categories				

Across these five languages, which are quite unrelated, the semantic domains of the realis-irrealis distinction encountered in Amele is still quite coherent. The only difficulty is that, whereas in Amele, Nobonob, and Wojokeso, past habitual is grouped in the realis domain. In Bargam this category is grouped in the irrealis domain. In response to BPP (63) I would not say that the Bargam case contradicts the category of irrealis as found in the other languages cited nor is it 'whimsical' that in Bargam past habitual is categorized as irrealis. In fact, it is the nature of habituality that it can be classified as either an aspect or a mood. For example, Comrie (1985:39) cites Dyirbal as a language that has two basic finite forms of the verb. One is used for actual situations, i.e. situations that can be identified as happening at the present or having happened in the past; the other is used for situations that are not subsumed by the first form, i.e. situations that are predicted for the future, and situations that are induced to be general even

though they are not observable as ongoing at the present moment or as having occurred in the past. In Dyirbal this second form would be used for habitual statements. So Comrie argues that the distinction made in these verb forms is not one of future versus nonfuture tense as Dixon (1972:55) states but is rather one of realis versus irrealis modality. In Dyirbal then, habituality is a modal notion. Also Dahl (1985:101-102) noted in his cross-linguistic survey of tense and aspect systems that, while in general the meaning of past habitual could be described as a combination of habituality and past time reference, at least two languages in his survey, Bengali and Azerbaijani, exhibited a secondary use of past habitual and that was in counterfactual sentences, a modal context.

Comrie (1976:27-28) defines habituality as describing a situation that is characteristic of an extended period of time. In this sense habituality is an aspect since it views a situation as characeristic of a period of time. However, habituality can also be conceptualized as a modality in contrast to a tense. A tense, e.g. past, present, or future, locates an event deictically to the moment of speech, either directly or indirectly. However, with a habitual expression such as 'Fred is always beating his wife', the situation is not located before, after, or simultaneous with the moment of speech. Rather it is generalized to all time points and therefore can be conceptualized as no longer relating either directly or indirectly to the moment of speech and therefore no longer relating to the 'real world'. With past habitual the position is somewhat different since the notion of past time is obviously implied in the meaning. However, past habitual can also be conceptualized as modal in the sense of whether the situation described as habitual in the past can be related to the circumstances that hold at the moment of speech or whether the situation described as past habitual is considered to be another time, another set of circumstances that no longer hold in the present. In this case the past habitual would be conceptualized as being in an irrealis domain.

Comrie (1985:40) claims that no language will indicate habituality by means of a tense opposition but that the grammatical expression of habituality will always be integrated into the aspectual or modal system of a language rather than into its tense system. This claim is born out in both the Amele and Bargam cases. In Amele past habitual is an aspect and not a tense. As already mentioned, all the past tenses in Amele, today's, yesterday's and remote past tense, collapse into one negative past tense form. However, the past habitual has its own negative form distinct from the negative past tense form. In Bargam past habitual is classified as a modality by the medial SR verb.

BPP's criticism (63):

> ...that if this binary distinction differs so much across languages that a past tense, which is usually considered one of the prototypical cases of realis (Foley, 1986:158f), can be irrealis in some languages, this binary distinction is not cross-linguistically valid.

is therefore something of a straw man since past habitual is not a past tense.

3. The future tense connection. In the previous section I presented a recapitulation of the arguments for the category irrealis in the medial verb systems of the five Papuan languages—Amele, Nobonob, Anjam, Wojokeso, and Bargam. In this section I will show how future tense is connected to the irrealis category in Papuan medial verbs.

Dahl (1985:103-112), in his cross-linguistic survey of tense and aspect systems, noted that reference to future time differs epistemologically and probably ontologically from reference to the present and past. We cannot perceive the future or 'remember' it. We cannot report on events in the future as we can of events in the past or present. Normally when we talk about the future we are either talking about someone's plans, intentions, or obligations, or we are making a prediction from the present state of the world. Therefore Dahl say (185:103):

> As a direct consequence, a sentence which refers to the future will almost always differ also modally from a sentence with non-future time reference.

Even so, in Dahl's survey 'future' was one of the three categories that is most often marked morphologically. With respect to the semantics of the category 'future' Dahl says (1985:108):

> FUT can be best described in terms of a prototype involving at least the three features 'intention', 'prediction' and 'future time reference'.

With respect to maintaining the category 'future tense' Dahl found that 'future time reference' is a more constant element of FUT than the modal features of this category and that 'future time reference' could therefore be regarded as a dominant feature of FUT. Dahl (1985:111) also noted that for two Turkic languages—Azerbaijani and Turkish—habitual categories may be used secondarily in predictive (future time reference) contexts. The picture that emerges of Dahl's semantic category FUT is a prototypical category with 'future time reference' as the core or basic attribute which can occur in future-oriented modal contexts and also habitual contexts.

In a survey of SR in Papua New Guinea (Roberts, *forthcoming*) I found that out of the one hundred fifty-five languages in the survey one hundred nine had medial verbs marked for SR, i.e. 70 percent. It was also the case that these SR medial verbs are normally not marked overtly for categories such as tense or mood but rather these categorial specifications are read off the verb in the final clause and apply to all the verbs in the clause chain. In the survey it was found that in only twenty-seven languages with a SR system was a tense category (such as past, present or future), marked on the medial SR verb separately from the tense category marked on the final verb. In these cases it was still usually the case that the tense category marked on the medial verb was in concord with the tense category marked on the final verb.

For example, the Papuan language,Tauya (MacDonald, 1983), has a future versus nonfuture tense distinction marked on the SR medial verb, as illustrated in Table 4 (' is [ʔ]. In Tauya the SuAgr on the DS medial verb is the same form as that on the final verb. What distinguishes them as DS medial verbs is the suffixes *-te, -fe, -tefe*.

Table 4. Tauya medial verb nonfuture-future marking

verb stem +	SS: -pa				
	DS: NONFUTURE:	SuAgr	S	P	+ -te
		1/2	-e	-ene	
		3	-a	-i	
	DS: FUTURE:	SuAgr	S	P	+ -te
		1INC	Ø	-ame	
		1EXC	-amu	-anene	
		3	-'e	-'ai	
		2	-a		+ -fe
				-ane	+ -tefe

MacDonald (1983:116-117) reports that the SS medial verbs are not marked overtly for tense. If the final verb is aorist (nonfuture) tense a preceding SS medial verb is interpreted as being aorist. If the final verb is future tense a preceding SS medial verb is interpreted as also being future tense. This is illustrated by (25).

(25) a. *ne-ne fofe-pa ya-tu-a-'a*
3S-ERG come-SS 1S.O-give-3S-IND
He came and gave (it) to me. (Tauya)

b. *ne-ne fofe-pa ya-tu-'e -'a*
3S-ERG come-SS 1S.O-give -3S.FUT-IND
He will come and give (it) to me.

DS medial verbs, on the other hand, are marked for tense. Those with non-second person subjects are only marked for the aorist (nonfuture) tense and those with second person subjects can only be marked for future tense. In both cases the tense marked on the medial verb is neutralized and the medial verb is interpreted as being in the tense of the final verb, as illustrated by (26).

(26) a. *ne fofe-a-te Ø-tu-e-'a*
3S come 3S.AORIST-DS 3S-give-1/2S.AORIST-IND
He came and I gave it to him.

b. *ne fofe-a-te Ø-tu-amu-'a*
3S come-3S.AORIST-DS 3S-give-1S.FUT-IND
He will come and I will give it to him.

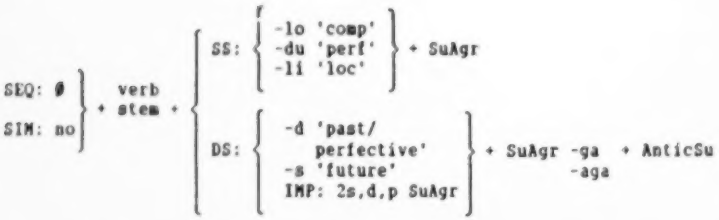
c. *na momune-a-fe ya-ne pofei-ti na-tu-e-'a*
2S sit-2S.FUT-DS 1S-ERG talk-CONJ 2S-give-1/2S.AORIST-IND
You sat and I talked to you.

d. *na momune-a-fe ya-ne pofei-ti na-tu-amu-'a*
2S sit-2S.FUT-DS 1S-ERG talk-CONJ 2S-give-1S.FUT-IND
You will sit and I will talk to you.

Tauya also has seven moods: indicative, interrogative, subjunctive, imperative, exclamatory, prohibitive and necessitative—each marked by a clause final sentence particle. As in Amele, none of these mood particles affect the tense marking on the medial verb. The future versus nonfuture distinction in the Tauya medial verb is therefore purely one of tense.

In Yagaria, too (Renck, 1975), there is a distinction marked in the DS medial verb between future versus nonfuture, as illustrated in Table 5. The *-s* 'future' marker only occurs with a final verb marked for the intentional future or regular future tense. Other tenses marked on the final verb are past and present. The medial imperative form occurs with a final imperative or future form. The medial imperative form is the same as the final form except in the final form there is also a verb final particle *-o* . So in Yagaria, as in Tauya, the future versus nonfuture distinction marked on the medial verb is purely one of tense since imperative mood is not included in the future tense category.

Table 5. Yagaria medial verb nonfuture and future forms.

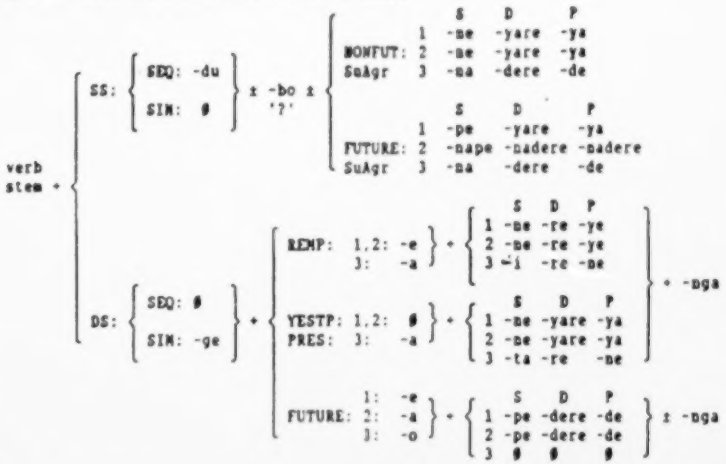


Haiman (1980) also reports that in Hua, a language belonging to the same dialect chain as Yagaria, the only tense distinction made is that of future versus nonfuture. There is also a DS medial future form distinct from a DS medial imperative form. However, Haiman (*personal communication*) advises that in Hua the future medial form can co-occur with a final imperative form so the future versus nonfuture distinction in the medial verb is not purely one of tense.

In the survey (Roberts, *forthcoming*) it was quite rare to find languages with SR medial verb forms that marked a future versus nonfuture distinction and for that distinction to be purely one of tense. Tauya and Yagaria were the only clear cases. More commonly a 'future tense' category marked on a SR medial verb would include a modal category, such as imperative mood.

For example, in the Erima language (Colburn, 1981) there are distinctive sets of SuAgr markers for the four final verb tense categories of present, yesterday's past, remote past, and future tense. However, for the medial SR verbs these categories are marked in a more neutralized form. In the DS medial verb there is a three-way distinction marked of 'past', 'present', and 'future'. The DS.PAST concords with remote past tense on the final verb, the DS.PRESENT concords with present and yesterday's past on the final verb, and the DS.FUTURE concords with future tense on the final verb. In the SS medial verb, however, there is just a two-way distinction marked of 'future' versus 'nonfuture' and this is optional for the SS medial verb. This is illustrated in Table 6.

Table 6. Erima medial verb tense marking.



Colburn (1981) gives a number of examples of 'future tense' medial forms occurring with verb final imperative forms. The verb final imperative form in Erima is marked with *-wa ≈ -a*. These examples are given in (27).

(27) a. *yapa-∅-bo-nape* *ny-a-u*
sit-SIM-?-2S.FUT.SS eat-IMP-2S
While you sit eat! (Erima)

b. *yapa-du-bo-nape* *ny-a-u*
sit-SEQ-?-2S.FUT.SS eat-IMP-2S
After you sit eat!

In Erima the distinction marked on the SS medial verb is not strictly a future versus nonfuture tense distinction since the modal category of imperative is included in the 'future tense' category. Erima also has a counterfactual mood final verb form but Colburn (*personal communication*) advises that the future medial form cannot co-occur with the final counterfactual form.

As noted in Roberts (1990) for a number of other Papuan languages with a SR medial verb distinction described as future versus nonfuture tense it could be shown that the 'future tense' form is actually modal since a category such as imperative is included in the 'future tense' category. An example from Gahuku (Deibler, 1976) is reproduced as (28).

(28) *ho NO-it-i-KO ano*
sun PROG-rise-3S-DS.FUT come.IMP
come as the sun is rising (Gahuku)

Also in Roberts (1990) was Angaataha (Huisman, 1973), an Angan language like Wojokeso, where the 'future tense' medial form includes desiderative and imperative mood. Ambulas (Wilson, 1980) is another Papuan language which has a marking on the SR medial verb described as 'future tense' which turns out to have a modal context. In Ambulas the SS and DS verbs can be marked for the categories of SEQ and SIM. The DS verb can also be marked with the future suffix *-u ≈ -o* which Wilson (1980:73-74) says can occur in either a future tense, desiderative tense, or imperative mood context.

(29) a. *kéraa-n-o y,-ké dé y-o*
get-1P-DS.FUT go-FUT 3S do-PRES
we will get and he will go. (Ambulas)

b. *akélak mé ra-n-o*
quietly IMP sit-1P-DS.FUT
we will sit quietly.

c. *vi nak mé yé gi-kwe-mén-u dawuli r-e*
spear one IMP go tie-3S.BEN-2S-DS.FUT go down sit-SS

sayéké viyaa-tiyaa-d-u baak-ne ka-ké wuné-k
cassowary strike-1S.BEN-3S-DS.FUT steam-SS eat-DES 1S-DES
you go and make a spear for him and let him go down and sit and kill the cassowary and let me steam-cook and eat (it).

A number of Papuan languages are cited in the literature as having a future versus nonfuture distinction marked on the medial verb. Kapau (Oates and Oates, 1968), another Angan language, has six tenses marked on the final verb: present, today's past, yesterday's past, remote past, historical past, and future tense. However, these tense distinctions are neutralized in the medial SR verbs. The medial DS verb neutralizes to just a past, present, and future distinction and the medial SS verb neutralizes to just a future versus nonfuture distinction. Gende (Aufenanger, 1952) is a language related to Gahuku with future and nonfuture medial forms similar to Gahuku. Other languages cited include: Awa (Loving, 1973), Boiken (Freudenberg, 1979), Botin (Pryor and Farr, 1989), Samo (Shaw, 1973), and Tairora (McKaughan, 1966). In each of these cases it is not clear from the descriptions given exactly what final verb categories co-occur with the medial verb 'future' and 'nonfuture' categories.

Another Papuan language cited as having a 'future' category marked on the SR medial verb should also be mentioned at this point. Reesink says that in Usan not all SR medial verb forms are devoid of tense indication and derive their tense from the first following final verb (1987:88):

> There is also a set of future medial forms. They signal that the time of the state of affairs they express is projected forward from the time determined by the first following final verb.

What these 'future' medial forms in Usan express is the notion of intention or purpose as illustrated by (30).

(30) a. *ani-mbege-ib* *qâmb* *di-aum*
you P.O-see-S.FUT.SS say.SS come up-1S.PRES
I have come up to see you. (Usan)

b. *wuri uru uyo-ub-ari* *ne ye nob ir-amei*
they dance sway-FUT-3P.DS and I with ascend-1S.FP
They were going to dance and I went up with them.

The function of these 'future' forms in Usan is therefore different from the function of the 'future' forms we have already examined. In all the other Papuan languages cited so far as having a SR medial verb with a future form there is concord between these forms and the following final verb forms with respect to tense and modality. In Usan the SR medial future form does not concord with the tense expressed on the final verb since this medial future form can co-occur with any final verb tense category. Usan is therefore similar to a number of other Papuan languages, such as Ömie (Austing and Austing, 1977), Agarabi (Goddard, 1980), Fasu (Loeweke and May, 1980) and Kobon (Davies, 1981), of which there is a SR medial verb form expressing purpose or intention.

After examining how tense is marked in the medial verb of certain Papuan languages we see a pattern emerging. It is comparatively rare for a Papuan language with a SR medial verb system to mark tense categories, such as past, present, and future, on the medial verb separately from the tense categories marked on the final verb. It is more common in these languages to either not mark any tense category on the SR medial verb or mark the relative tense categories of sequential and simultaneous tense. In my survey (Roberts, *forthcoming*) I found that out of the one hundred nine languages that had a medial verb system with a marked SS/DS distinction in sixty-five languages a distinction was also marked of SEQ versus SIM in the medial verb. Nevertheless, in some Papuan languages tense categories such as past, present, and future are marked on the SR medial verb.

For some languages, such as Binandere (Capell, 1969), Daga (Murane, 1974), Enga (Lang, 1975), Fore (Scott, 1978), Koita (Dutton, 1975), Suena (Wilson, 1974), Washkuk (Kooyers, 1975), and Zia (Wilson, 1969), the tense categories marked on the medial verb are the same as those marked on the final verb. For other languages these tense categories when marked on the medial verb are neutralized in some way from the categories as found on the final verb. A common neutralization in the medial verb would appear to be to a future versus nonfuture distinction and in this section we have examined the semantics of this 'future tense' category in a number of languages.

The pattern that emerges is as displayed in Table 7. For some languages, viz. Tauya and Yagaria, the future tense as marked on the medial verb is a pure future tense and is not used in modal contexts. We then have other languages where the future tense has been grouped with other modal categories via the medial verb classification. In the first of these groupings are Erima, Gahuku, Angaataha, Ambulas, and Hua, where future-time oriented modalities such as imperative and desiderative are grouped with the future tense proper. In the next group are Amele, Nobonob, Anjam, and Wojokeso, where the development of the 'future' medial verb category is taken a step further. In these languages a nonfuture-time oriented modality such as counterfactuality is also grouped with future tense and imperative mood. Finally there is Bargam where past habitual on the final verb is also classified as a modality by the medial verb desinence.

Table 7. The irrealis category groupings of Papuan medial verbs.

TENSE - - - >	MODALITY - - - - - - - - - - >		
+future tense	+future tense +imperative mood	+future tense +imperative mood +counterfactual mood	+future tense +imperative mood +counterfactual mood +past habitual
Tauya Yagaria(DS)	Erima(SS) Gahuku Angaataha Ambulas(DS) Hua(DS)	Amele(DS) Nobonob Anjam Wojokeso	Bargam

The semantics of the category irrealis as found in Papuan medial verbs therefore turns out to be very similar to the semantics of Dahl's semantic category FUT. Irrealis is a prototypical category with 'future time reference' as the core or basic attribute which can occur in future-oriented

modal contexts such as imperative and desiderative, and also in nonfuture-oriented modal contexts such as counterfactual and habitual. Put another way, irrealis is the extended modal use of future tense.

Givón (1992:45-46) also maintains that irrealis has future time orientation as its core meaning:

> A careful study will reveal that the great bulk of the 'disparate' clause-types traditionally grouped under irrealis have a considerable measure of coherence and commonality:
>
> (a) They tend to be future-projecting ('not-yet-real').
> (b) They allow non-referring interpretation of NPs under their scope.
> (c) They tend to group into two broad sub-modal clusters, epistemic and valuative-deontic.
> (d) Whether epistemic or deontic, they all tend to involve interaction under **low certainty**.
> (e) Unlike realis, they tend to involve more **flexibility** of modal perspective in the interaction with the interlocutor).

4. The origins of irrealis in Papuan medial verbs. From the data presented in Section 3 it would seem most likely that the irrealis forms found in the SR medial verbs of Papuan languages have arisen as the basis for categorial concord between these verbs and the final verbs have altered diachronically. Medial verb forms are probably derived from final verb forms and originally their categorial inventories would have been the same. For example, in many Papuan languages the DS medial verb form is identical to the final verb form with some additional marking to indicate DS. In Enga (Lang, 1975), the DS verb is a final verb with an additional suffix *-pa*. In Suena (Wilson, 1974), the categories SEQ versus SIM are marked on the DS medial verb by sequencing the final verb tense categories. Suena has six tenses that can be marked on the final independent verb: present, today's past, yesterday's past, past (within the previous two years), remote past (prior to the previous two years), and future tense. Tense can also be marked on the DS medial verb but is restricted to the present, today's past, remote past, and future tense forms. These tenses can occur in combination on the DS medial verb and final verb to express the notions SEQ and SIM. This is illustrated in Table 8.

Table 8. Suena tense sequencing.		
DS-Medial Verb Tense	Final Verb Tense	
today's past	present today's past yesterday's past past (two years)	S E Q
remote past	remote past	
future	future	
present	present today's past yesterday's past past (two years) remote past future	S I M

The tense on the DS medial verb is neutralized with respect to its absolute meaning and instead expresses a relative tense notion with respect to the tense on the final verb. The sentences in (31) illustrate this function.

(31) a. *Na ge ses-e-n-a bamu-s-i-a.* (TODP + REMP = SEQ)
1S talk say-TODP-1S-IND go-REMP-3S-IND
When I spoke he left.

b. *Gi pupi-no-n-a pu bam-β-i-a.* (PRES + TODP = SIM)
spear get-PRES-1S-IND pig go-TODP-3S-IND
While I was getting my spear the pig went away. (Suena)

With most Papuan languages, as new categories were added to the medial verb forms (such as SEQ versus SIM, SS versus DS and so forth, which were not exponents on the final verb forms), the final verb categories marked on the medial verb forms would have been reduced. The exponence of these final verb categories on the medial verbs would then have been by concord only. In most Papuan languages the final verb categories are deleted entirely from the medial verb forms. In some cases the final verb category marked on the medial verb is neutralized in meaning. One type of neutralization is where several degrees of past tense marked on the final verb are neutralized to a single 'past' tense marker on the medial verb, as in

the Kapau DS medial forms. In Erima, present tense and yesterday's past tense are neutralized to a single 'present' form on the DS medial verb. In Wahgi (Phillips, 1976) this kind of medial verb tense neutralization has a slightly different twist. This language has the verb final tense categories marked of remote past, immediate past (which includes today and yesterday), present, and future, and the medial verb is marked for what may be termed a 'past versus nonpast' distinction. The final verb category immediate past tense can co-occur with either the past or nonpast medial verb form depending on the context as illustrated in Table 9.

Table 9. Co-occurrence of Wahgi medial and final verb tense categories.

		Medial verb tense categories:	
		PAST	NONPAST
Final verb tense categories:	remote past	X	
	immediate past (yesterday-today)	X	X
	present	X	
	future	X	

Comrie (1985:102-107) distinguishes tense neutralization from tense sequencing. In tense neutralization one verb at the beginning of a string of verbs is marked for the tense to be expressed. The subsequent verbs in the string will either be unmarked for tense or will be marked by a single tense category which is neutralized by the tense category marked on the first verb in the string. So that in effect all the verbs in the string express the same tense category as the first verb. An example from English would be *I will go to the shop and buy some bread.* Future tense is only expressed overtly by the auxiliary *will* in conjunction with the first verb *go* but future tense also applies to the second verb in the string *buy*. Tense neutralization can be characterized as a type of agreement operating across clauses at the same structural level, i.e. in a coordinate relationship. Tense sequencing operates within a particular syntactic construction. In a given structure a particular tense will be required to be marked on a subsequent verb in a series in order to express the meaning. An example of tense sequencing in English would be in an indirect command, such as *I told him to eat his dinner.* Here the subsequent verb must be a *to* infinitive in order to express the indirect

command. The crucial difference from tense neutralization is that in tense sequencing the tense expression on the subsequent verb is usually different and independent from the tense expression on the first verb. Tense sequencing can therefore be characterized as a type of government where the tense-form of one verb in a given syntactic construction requires a particular tense-form of another verb which is structurally subordinate to it.

In Comrie's terms the situation in PNG languages where the tense-mood marked on the final verb applies to all the verbs (clauses) in the clause chain would be a clear case of tense neutralization, although in this instance the controlling verb occurs at the end of the string instead of at the beginning as characterized by Comrie. Instances of tense sequencing in Papuan languages would be in the case of Usan and the other languages cited that mark a 'future' tense on the medial verb which is independent of the tense or mood marked on the final verb. The notion of tense sequencing would also apply in the Suena case where sequences of final tense categories are used to express the notions of SEQ and SIM relative tense.

The most common neutralization of tense distinctions in Papuan medial verbs would appear to be a future versus nonfuture distinction. This is probably because of the more basic epistemological difference between relating future and nonfuture events. Once a future versus nonfuture distinction is obtained then the 'future' category is readily extended to include other categories which describe some non-existing or not yet existing state of affairs. This would include first of all future-time oriented modalities and then nonfuture-time oriented modalities.

5. Conclusion. Thus the category irrealis, as marked in the medial verb forms of a number Papuan languages including Amele, can be analyzed as a prototypical category with the core meaning of referring to an event that has not yet happened. This core meaning, based on the category of future tense, has been extended metonymically within a broader conceptual structure to include other modal categories with a related meaning.

On the basis of the different irrealis groupings of final verb categories illustrated in Table 7 it was suggested that the means by which the medial verb irrealis category has developed in these languages has been through a process of tense neutralization in the medial verb. The hypothesis presented is that different contrasts of past, present, and future tenses in the medial verb have perhaps initially neutralized to a simple past, present, and future contrast and then neutralized to a future versus nonfuture contrast while maintaining concord with the final verb categories. This hypothesis, although speculative since there are no historical records of earlier forms of

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these languages available, is quite plausible once the nature of the medial verb realis-irrealis marking is understood as a system of agreement or concord between final verb forms and medial verb forms.

With all the Papuan languages examined that marked a realis versus irrealis distinction in the medial verb it was found that verbal negation did not affect the realis-irrealis marking. In each language an event can be described as either positive or negative realis or positive or negative irrealis. The medial verb realis-irrealis marking therefore indicates the degree of commitment the speaker has to the truth of the proposition as a whole. If it is a realis proposition it is in a conceptual domain that can be reported upon, so the speaker can either affirm belief or disbelief in the proposition. An irrealis proposition is in a conceptual domain that cannot be reported upon and therefore the speaker cannot affirm belief or disbelief in the truth of the proposition. With regard to negation it is interesting to note that in Central Pomo, a North-American Indian language with a realis-irrealis distinction marked on the SR medial verb similar to the Papuan languages cited, it is also the case that negation is separate from the realis-irrealis modal system.

In fact, the realis and irrealis categories of Central Pomo and Amele are remarkably similar. As shown in Table 10 the only major difference is that in Central Pomo a future tense form can co-occur with a medial realis form. This expresses the speaker's certainty of a future event.

Table 10. Realis and irrealis categories in Central Pomo and Amele.

	Central Pomo	Amele
Realis:	present tense	present tense
	past tense	all past tenses
	past habitual	past habitual
	future tense	
Irrealis:	future tense	all future tenses
	imperative	imperative
	hortative	hortative
	hypothetical	apprehensive
	counterfactual	counterfactual
		purpose

It is also the case that the core irrealis categories found in Amele and Central Pomo of future-hypothetical match almost exactly the core irrealis categories of Austronesian languages determined by Bugenhagen (1991). If

there was no cognitive validity to the category of irrealis then it would be difficult to explain how these close similarities of category groupings can arise in these three quite distinct language families.

If we assume that all linguistic categories are prototypically defined in terms of the attributes of the core members of the category then the fact that the category irrealis is not consistent across languages should not cause any difficulty. Rather irrealis is an excellent example of a prototype category with a core meaning of relating to future oriented events that have 'not yet happened' or are 'not yet real'.

Appendix. Alphabetical list of languages cited†

Language	Family	Location
Agarabi	Kainantu	E. Highlands, PNG
Ambulas (Abelam)	Ndu	E. Sepik, PNG
Amele	Gum	Madang, PNG
Angaataha	Angan	Morobe, PNG
Anjam (Bom)	Mindjim	Madang, PNG
Awa	Kainantu	E. Highlands, PNG
Azerbaijani	Turkic	Azerbaijan
Bargam (Mugil)	isolate	Madang, PNG
Bengali	Indo-Iranian	India
Bilibil	Belan	Madang, PNG
Binandere	Binandarean	Morobe, PNG
Boiken	Ndu	E. Sepik, PNG
Botin (Kambot)	Grass	E. Sepik, PNG
Bouomaa Fijian	Austronesian	Fiji
Bukiyip	Torricelli	E. Sepik, PNG
Caddo	Caddoan	Texas, U.S.A.
Central Pomo	Pomoan	N. California, U.S.A.
Daga	Dagan	Milne Bay, PNG
Dyirbal	Australian	N. Queensland
Enga	Engan	Enga, PNG
English	Indo-European	England
Erima	Nuru	Madang, PNG
Fasu	W. Kutubuan	S. Highlands, PNG
Fore	Gorokan	E. Highlands, PNG
French	Indo-European	France
Gahuku	Gorokan	E. Highlands, PNG
Gedaged	Belan	Madang, PNG
Gende	Gorokan	E. Highlands, PNG
Hua	Gorokan	E. Highlands, PNG
Italian	Indo-European	Italy
Kapau	Angan	Morobe, PNG

Kobon	Kalam	Madang, PNG
Koita	Koirarian	Central, PNG
Manam	Austronesian	Madang, PNG
Matukar	Belan	Madang, PNG
Menya	Angan	Morobe, PNG
Muyuw	Austronesian	Milne Bay, PNG
Nobonob (Garuh)	Hanseman	Madang, PNG
Ömie	Angan	Oro, PNG
Salt-Yui	Chimbu	Chimbu, PNG
Samo	E. Strickland	Western, PNG
Sinaugoro	Austronesian	Central, PNG
Suena	Binandarean	Morobe, PNG
Sursurunga	Austronesian	New Ireland, PNG
Tairora	Kainantu	E. Highlands, PNG
Takia	Belan	Madang, PNG
Tauya	Brahman	Madang, PNG
Tigak	Austronesian	New Ireland, PNG
Turkish	Turkic	Turkey
Usan (Wanuma)	Numugenan	Madang, PNG
Wahgi	Chimbu	Chimbu, PNG
Washkuk (Kwoma)	Nukuma	E. Sepik, PNG
Wojokeso (Ampale)	Angan	Morobe, PNG
Yagaria	Gorokan	E. Highlands, PNG
Zia	Binandarean	Morobe, PNG

† Alternative language names are given in parentheses.

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THIRD ANNUAL WORKSHOP ON COMPARATIVE LINGUISTICS
Purdue University—November 12-13, 1994
Topic is 'subgrouping'. Sessions will cover Methods of Subgrouping, Southeast Asian, Afro-Asiatic, Indo-European, and Germanic.
Contact Mary Niepokuj (niepokuj@mace.cc.purdue.edu).

+ ANNOUNCING A NEW JOURNAL +

Lexikologie - Lexicology - Lexicologie, an International Journal on the Structure of the Vocabulary. **Editors**: Peter Rolf Lutzeier, Guildford (Managing Editor); Dmitrij Dobrovol'skij, Moscow; Adrienne Lehrer, Tucson; Hans-Juergen Sasse, Koeln. **Topics**: Theory and practice of vocabulary structure in natural languages, Mental lexicon, Lexicon in grammatical theory, Lexicon in computational linguistics, History and methodology of lexicology. Two issues per volume. First issue planned for spring/summer 1995. Orders may be directed to Walter de Gruyter, Berlin.

Call for Papers: Four copies of manuscript (after Jan. 1, 1995)—English, German or French accepted. Contact Hans-Juergen Sasse, Universitaet zu Koeln, Institut fuer Sprachwissenschaft, D-50923 Koeln, Germany.
E-mail: am000@rsl.rrz.uni-koeln.de; Fax: +49-221-470 51 58.

NOTICE OF CHANGE IN EDITORSHIP FOR TWO JOURNALS

Cognitive Linguistics. New Editor-in-Chief Arie Verhagen (Utrecht) took over from Dirk Geeraerts (Leuven), on Sept. 1994. The new editorial address and contacts are: Arie Verhagen; Centre for Language and Communication, Utrecht University, Trans 10, 3512 JK Utrecht, The Netherlands.
Phone: +31 - 30.53.8131; Fax: +31 - 30.53.6000; E-mail: coglx@let.ruu.nl.

Typological Studies in Language (TSL). Mickey Noonan will take over from Tom Givón as next General Editor beginning Jan. 1996. Two associate editors will work with him: Suzanne Kemmer and Spike Gildea (both of Rice). During the interim period of 1995 Spike Gildea will be Associate Editor.

CONFERENCE ON FUNCTIONALISM AND FORMALISM

New date for conference has been set for April 18-20, 1996 (Thursday-Saturday), on the University of Wisconsin-Milwaukee campus. A new call for papers will be posted in Spring, 1995, with the abstract deadline set for Nov. 17, 1995.
Contact Edith Moravcsik (edith@csd.uwm.edu) or
Michael Noonan (noonan@csd.uwm.edu)

Language underived: A diary entry

John Verhaar
Indonesian Linguistic Development Project, University of Leiden

The scholarly paradigm obtaining for linguistics is changing. It is perhaps easiest to find out what that paradigm is by considering first what it has been changing from, before considering what it has been changing into.

The conventional paradigm has been to see language as derived; there are various ways of phrasing that. One is this: language is derived from thinking, or from the 'mind'. The Cartesian model 'I think, therefore I am' has become something that could be phrased as 'I speak, therefore I am'. What was new in the original Cartesian notion was the 'I', spawned by the (early seventeenth century) change from philosophy as a quest for truth to philosophy as a quest for certainty—a change (culturally) from 'What about truth?' to 'What about me?' Apart from that introduction of the 'I' or 'Ego', the rest was pre-Cartesian—as we may see in the medieval *grammatica speculativa*: grammar is a 'mirror' of what goes on in the intellect. Thomas Aquinas had already considered what we now call a 'concept' as a *verbum internum*, an 'internal word'. If Aquinas had had a philosophical notion of 'I' (which he did not), he could have said 'I speak, therefore I think'.

1. There have been other paradigms, of course. The Darwinian nineteenth century had a developmental notion of language: language is derived from its former states. Also, in Wilhelm Wundt's work, language was placed in a position where it derived from 'psychology'. In our century, a born-again Saussure moved diachronic concerns to second place and started to work synchronically, and in the process he moved 'thinking' as well as things we speak ABOUT, or (say) 'the world', to the domain of the 'extralinguistic'. Language had become a texture all its own, a 'structure' of 'structures'. Underived, therefore? Not really, for Saussure considered linguistics as belonging to 'semiology', which in turn belonged to 'psychology'. In North America, Bloomfield picked up the Wundtian theme in his *Introduction to the study of language* (1914), and introduced in his *Language* (1933) what amounted to a behaviorist variety of 'structuralism': 'meaning' was ousted, and so was that 'world'. Was THAT one underived? Not really—Bloomfield's inspiration came from 'psychology' as well.

Psychology has remained victorious in 'mentalism'—Chomsky sees linguistics as part of 'psychology'. Indeed, he does not think language divorced from its mental source is interesting. That 'world' is still excluded, as in Bloomfield and, in fact, paradigmatically, mentalism may be no more than the Platonization of the Bloomfieldian paradigm. As Simon Dik once said, mentalism needs 'deep structures' only because the 'surface structures' are still Bloomfieldian. In short, what you and I say (or would, as ideal speakers, if we eliminated false starts, throat clearings, 'oh's, ah's and uh's', stuttering, anacoloutha, the parenthetical, malapropisms and other errors the 'mind' can never plead guilty of) derives from what in a spate of mixed metaphors is supposed to be 'deeper', or in a 'higher' node, or due to 'earlier' rules. But ultimately, or primarily, there's only one source of derivation: the mind ('ultimately' if you work backwards from utterances, 'primarily' if you only pretend to do so).

2. So far, no 'underived' paradigms. Are there any? Not in most studies I see. Recently I read one using predicate logic to have 'representations' of clauses and sentences. In the formalization of predicate logic, the 'predicate' precedes the argument(s), and I sometimes have to pinch myself to maintain awareness that this is not (only) about verb-initial languages (my problem, of course, not the author's). As in predicate logic, in this linguistic study, too, nouns 'derive' from (pre-expression) predicables, and only in derived (and expressed) form are they either predicative grammatically or bare nouns (for example, as arguments to a grammatical predicate). So the lexicon is a fund of pre-expression predicative structures. Really? I have to pinch myself again (not my problem, this time).

One cannot have derived paradigms without ultimate (or primary) non-negotiables. On the other hand, one cannot insist on underived paradigms with making THAT nonnegotiable. So where does that leave the linguist?

3. Now, those non-negotiables need not really be a problem. In Relational Grammar, subjects and objects are 'primitives'. Does anything more basic underlie them? No. Well, never mind, let's pretend nothing does. Then we do make discoveries, especially in RG—which has a far-flung cross-linguistic data base. Contemporary linguistics would not be where it is now without RG, *inter alia*. But the same is true of generativists—most linguists in that school would not give two hoots about Cartesian (or

Aristotelian) ancestry. But generativists, too, have changed the linguistic scene. Perhaps even predicate logic may be a vehicle that takes us somewhere (or perhaps class logic, or propositional logic?).

There is some appeal in what Charles Fillmore once called the serious job done by 'the ordinary working grammarian', and there is perhaps no need always to do instant prospecting for what may be mined underneath. One can be a Chomskyan, I suppose, and still take genuine interest in the work of the scientist who has tried to make a chimpanzee speak, even though, naughtily, she has named her pupil 'Chimsky'. Or one may be committed to non-Chomskyan positions and still feel skeptical about the logocization of primates.

Thus, even non-negotiable positions may be made to look comparatively innocuous.

4. Some linguists are now engaged on projects which have sworn off what most underlying assumptions of various schools have in common. The kind of English data used by grammarians is now occasionally called a 'mythical' kind of English, and indeed it is fairly rare even if not quite indisputably mythical. It is now asked how people actually talk. The 'language-is-just-talk' school is also getting somewhere. Once one does this kind of work, it seems irrelevant to start 'justifying' the wider frame of reference implicit in doing the job—whether psychologically, logically, or philosophically.

But should I add 'philosophically'? Philosophy itself is now changing—and deprofessionalizing itself by becoming aware of its links to literature. Philosophers like Richard Rorty would redefine a philosopher as an 'all-purpose intellectual' and they do not recognize anything outside language. They work with 'discourse', which is locked into the conventions of prior discourse. Rorty feels that the 'justification' of a philosophical theory is nothing but the description of how social conventions intellectually have led to the position vindicated.

Functionalists among linguists have a comparable inspiration. They are interested in how discourse exerts pressures on grammar formation—the process of what Paul Hopper calls 'emergent grammar'. Thus, grammar is not autonomous. Neither is discourse, of course, and perhaps nothing founds anything in ways we have access to without encountering more formidable 'no entry' signs. But if one doesn't worry about the lack of that

access, a huge amount of revealing work can be done. If we pretend language is underived and enjoy the lack of conceptual overhead, we will have contributed something to efforts to free ourselves from a rationalistic past. That past has never given what it promises, and probably never will.

■

Call for Papers

⌘ SYMPOSIUM on LANGUAGE LOSS AND PUBLIC POLICY ⌘

in conjunction with the

1995 Linguistic Institute of the Linguistic Society of America

University of New Mexico, Albuquerque, NM, USA

June 30-July 2, 1995

The Symposium on Language Loss and Public Policy will bring together scholars from different disciplines to discuss the linguistic, psycholinguistic, sociolinguistic, cultural, and policy aspects of language loss. LANGUAGE LOSS is used here in its broadest sense to subsume three areas of investigation:

(1) the ATTRITION of native language skills by individual members of indigenous and immigrant communities;

(2) societal SHIFT from the use of the native ethnic language to the use of a dominant official language; and

(3) the consequent DEATH of the subordinate language.

The clear interrelationships among these three areas—in the linguistic processes involved and especially in the societal conditions that give rise to loss—gives a meeting such as this Symposium considerable scholarly significance. The fact that the incidence of linguistic and cultural disruption worldwide is rapidly accelerating also makes the need for such a Symposium pressing.

Central objectives of the Symposium will be, in light of what is known about language loss, to examine its ecological significance, that is, its effects on individuals, communities, and society as a whole, as well as the policy implications of what is now seen to be a worldwide and rapidly accelerating phenomenon. Another important objective is to provide information to members of the wider community both as an educational objective and as a resource for those concerned with questions of policy.

✧ **Deadlines** ✧

Intention to submit formal abstract: Nov. 1, 1994. Include tentative title, specification of language situation, area of language loss emphasized, indication of focus, either on nature and causes of loss, or, on consequences of loss and policy implications.

Formal Abstracts: Jan. 31, 1995. *Pre-pub. version of papers*: May 15, 1995

Expressions of interest, formal abstracts, and requests for information should be directed to: Garland D. Bills, Dept. of Linguistics, Univ. of New Mexico; Albuquerque, NM 87131-1196 USA.

Telephones: (505) 277-7416 or 277-0324. Fax: (505) 277-6355

E-mail: gbills@bootes.unm.edu.

The 19th International L.A.U.D. Symposium

Language and Space

Eugene Casad
SIL—Mexico Branch

The 19th International Symposium of the Linguistic Agency of the University of Duisburg was held in the newly remodeled Wolfsburg Catholic Conference Center in Duisburg, Germany during the week of March 22-26, 1993. The symposium was actually a double barreled event with the major symposium being 'Language and Space' and the minor one being 'Conditionals & Co.'

The conference was held three days with keynote lectures by the following: Ron Langacker, UCSD: 'Viewing'; Debbie Hill, Max Planck Institute-Nijmegan: 'Distinguishing the notion PLACE in Oceanic languages'; John Taylor, University of the Witwatersrand-Johannesburg: 'The syntax and semantics of the Zulu locative'; Ewald Lang, Berlin: 'Basic dimensions terms: A look at universal features and typological variation'; Steven Levinson, Max Planck Institute Research Group for Cognitive Anthropology, Director: 'Language and spatial conception'; Dirk Geeraerts, Catholic University of Leuven: 'A typology of salience phenomena in the spatial lexicon', and Günter Senft: Max Planck Institute: 'A man, a tree and forget about the pigs: Space games, spatial reference and cross-linguistic comparison'.

Beyond that, there were 45 general session lectures, including my own, where I presented the paper, 'What good are locationals, anyway?' Particular session lectures that I really liked included Carlos Inchaurralde: 'Space and movement in the English verb', Brygida Rudzka-Ostyn: 'The structure of the genitive category' and Chris Sinha and Tania Kuteva: 'Spatial structures developing into the progressive: Cross linguistic variation'.

As always, the Duisburg Conference was very worthwhile. This time I touched base with Professor Paul Werth of the University of Amsterdam, who is very interested in relating Discourse Analysis to the Cognitive approach to language, a topic I had explicitly mentioned in my paper. A selection of papers from the conference proceedings will be edited by Martin Pütz of the University of Duisburg. ■

Reviews

Theory and description in generative syntax: A case study in West Flemish. By Liliane Haegeman. 1992. Cambridge Studies in Linguistics, Supplementary Volume. Cambridge: Cambridge University Press. Pp. xviii, 244. $59.95.

Reviewed by Gert de Wit
SIL—Africa Area, Nairobi

Many readers of this journal are working on the grammatical descriptions of little known languages. For this task a whole range of theoretical models and theories are used, depending on the training the researchers have received and the level of intrinsic interest in syntactic structure that they have. Whatever model is used, it is worthwhile and enlightening to read descriptions written by formal linguists. The book under review shows the descriptive power of one type of formal linguistics—generative grammar.

In this readable book, Haegeman uses generative grammar in its more recent version, 'Government and Binding Theory' (GB Theory), to describe two phenomena in her own dialect—West Flemish, spoken in Belgium. West Flemish is closely related to Dutch. Haegeman is professor of English Linguistics at the University of Geneva and has written a number of books and articles within the framework of GB Theory—for example, the textbook, *An introduction to government and binding theory* (Oxford, Blackwell). Those who are not familiar with GB Theory do not need to study the textbook to be able to follow the description of West Flemish grammar. Chapter 1 (p. 44) gives a short introduction to GB Theory, which enables the reader to follow the description and theoretical discussion in the subsequent chapters. Another aid to the reader is a four-page glossary containing a list of technical terms The book has an index and an up-to-date bibliography. The text includes many references to specific books and articles for further reading.

The two topics discussed are (1) the doubling of subject pronouns and (2) the order of verb phrase constituents, in particular verb raising and verb projection raising. Chapter 2 is devoted to the first topic, chapter 3 to the second. Both topics have been researched earlier by Haegeman and others. In this book, she reanalyzes the data in the light of the latest developments in GB Theory. Chapter 4 has conclusions and topics for future research.

1. The doubling of subject pronouns. In West Flemish, the subject pronoun can be doubled with a subject clitic. For example:

(1) *Ze goa ze zie kuopen* (Haegeman's 52a)
she goes them she buy
subj obj full
clitic clitic pronoun
She is going to buy them

(2) *da- se zie komt* (Haegeman's 62a)
that subj full comes
clitic pronou
that she comes'

The clitic ze and the subject pronoun zie in (1) can occur without one other. The clitic cannot co-occur with a lexical NP, e.g. 'Rosemary' or 'the traveller'. It is also not possible to have two subject pronouns in one clause or to combine a pronoun with a lexical NP. When the pronoun doubles with the clitic, it is necessary to know which of the two is the subject and what are their structural positions in the clause.

In her discussion of these and other questions, Haegeman makes it relatively easy for the reader to follow her line of reasoning. She does not jump to conclusions without having made clear how she arrived at them. With respect to the questions mentioned above, she answers the first question by the second. In GB Theory, 'subject' is a notion that can be defined structurally.

Haegeman compares the West Flemish data with similar cases of subject doubling in French, Spanish and Italian. For example, in the simple Italian sentence

(3) a. *Io parlo italiano* (Haegeman's 64a)
I speak Italian
b. *Parlo italiano*
I speak Italian

the subject pronoun can be left unexpressed, as shown in (3b). The verb itself has sufficient inflection to identify the subject. In the same way, the subject clitic in West Flemish together with the verb gives enough information about number and person to make the presence of a full subject pronoun optional. The subject clitic would receive the thematic role (agent in this case) from the verb, while the pronoun is an expletive having an

emphatic function. Haegeman proposes that, as in Italian, the subject pronoun in West Flemish occupies the structural subject position.

This implies that the pronoun should be seen as the subject of the sentence. The clitic originates within the verb phrase, so that it can be assigned the necessary thematic role, and is then incorporated in the complementizer position. The incorporated clitic, the pronoun and the empty position within the verb phrase where the clitic came from form a co-indexed chain. This chain is permitted because it has both case and a thematic role.

Haegeman assumes that in case of doubling, the subject pronoun is base-generated in the subject position. In GB Theory, this is a position to which no thematic role is assigned. When there is no doubling of the subject pronoun, there would be no clitic in the verb phrase. In that case, the pronoun would be base-generated in the verb phrase, receive its thematic role and then move to the structural subject position to pick up case. Base-generation of full pronouns in a non-argument position (i.e. a position to which no thematic role is assigned) is an assumption that needs further research. One piece of evidence may come from the observation that lexical NPs cannot co-occur with a subject clitic. If they did, a lexical NP would have to be base-generated in the non-argument position according to Haegeman's analysis. In the present GB framework, this is impossible.

The doubled subject pronoun is described as having an emphatic function. It is a pity that the author does not say how much emphasis this pronoun bears. When there is a pronoun and a clitic with the same reference, it would be expected that the clitic would be the emphatic element. For West Flemish, Haegeman argues quite convincingly that it is the other way around. It would have been helpful if she had ranked the sentences with doubling. Now the reader is left to assume that a sentence with only a clitic has least emphasis on the subject, followed by one with only a subject pronoun, followed by a pronoun doubled with a clitic. It is unclear where a lexical (indefinite) NP as subject would come.

2. The order of verb phrase constituents. The order of constituents in the Dutch and German verb phrase is well known as being rather free. One of the interesting facts is the place of the matrix verb in complex sentences: verb raising is a property common to Dutch and German. Verb projection raising, a West Flemish construction that does not appear in Standard Dutch, offers even more possibilities for re-ordering the constituents in the sentence. Of the following examples, (6a) illustrates verb raising, (6b) and (6c) verb projection raising. All three sentences are derived from the same

underlying structure (5). The meaning remains the same as in (5). (In Chapter 3, these examples are numbered as (2a-d).)

(5) *da Jan [Valère v zen wuf nen boek kuopen] deeg*
that Jan Valère for his wife a book buy made
that Jan made Valère buy a book for his wife

(6) a. *da Jan Valère vu zen wuf nen boek deeg kuopen*
that Jan Valère for his wife a book made buy
b. *da Jan Valère nen boek deeg vu zen wuf kuopen*
that Jan Valère a book made for his wife buy
c. *da Jan Valère deeg nen boek vu zen wuf kuopen*
that Jan Valère made a book for his wife buy

In cases of verb raising, as in (6a), the causative verb of the matrix clause (*deeg* 'made') and that of the lower clause (*kuopen* 'buy') form some kind of clause union where the lower VP and the higher VP are merged. But in sentences like (6b) and (6c), it seems as if the word order in West Flemish is very free, not only within the clause but even across clause boundaries. It has indeed been argued that languages like West Flemish are non-configurational.

Because of this, Haegeman's treatment of verb projection raising may be especially of interest to those who work in (seemingly) nonconfigurational languages. Her discussion shows that even this variation in word order can be described in a principled way. For this, she uses the adjunction analysis and den Besten's and Webelhuth's approach to scrambling. Space does not allow summarizing the analysis in depth, but suffice it to say how sentences like (6b) and (6c) are derived from their base structure in (5); (6c) is simply a case of adjunction. The VP *nen boek vu zen wuf kuopen* is right adjoined to the VP of the matrix clause. In (6b), the NP *nen boek* is first scrambled out of the VP and left adjoined to the matrix VP. After that, the lower *VP [t] vu zen wuf kuopen* is right adjoined to the VP of the matrix clause. (*[t]* indicates the trace of the scrambled NP *nen boek*.)

The reader of this chapter will also find a discussion on ways to distinguish between monoclausal versus multiclausal structures, on restrictions on scrambling, and on possible adjunction sites for the scrambled and adjoined constituents. For this, Haegeman used and developed further some of the latest ideas about the representation of tense and agreement in the clause structure.

3. Final remarks and recommendation. There are several small typos in the book. I mention a few for the reader's sake. On p. 88, the reader is referred to section 2.1. Probably this ought to be section 2.2.3.3. On p. 109, example (6)b. and c. are said to be instances of verb projection raising, whereas they exemplify verb raising. Good examples of verb projection raising are given under (8) on the next page. On p. 186, when sentence (155a) is discussed, the wording is confusing. It says: 'I assume that on the lowest cycle V_2 is adjoined to V_1 and the resulting unit is adjoined to V_3.' However, the following wording would give the desired results: '...on the lowest cycle V_2 is adjoined to V_1 and V_3 is adjoined to the resulting unit'.

These minor things, however, do not diminish the pleasure of reading this book. It is well written and organized. Although of a theoretical nature, the author tries to keep her discussion and analysis understandable for the linguists who are not familiar with GB Theory. For them, the introduction in Chapter 1 can be of great help. Those who are working on languages with subject doubling phenomena or complex verb phrase constituent order may find some helpful ideas. For those who are interested in the GB Theory itself or in possibilities to use generative syntax to describe little researched languages, this book will be of much interest.

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Haegeman, L., and H. van Riemsdijk. 1986. Verb projection raising, scope and the typology of rules affecting verbs. Linguistic Inquiry, 17, 417-66. ■

Call for Abstracts
GURT Pre-session on COMPUTER-MEDIATED DISCOURSE ANALYSIS
Georgetown University—March 8, 1995

One of the most exciting recent developments in discourse analysis is the availability of data from computer-mediated interactions, i.e. those that take place on the Internet, Usenet, and in synchronous modes such as Chat and MUDs/MOOs. **If interested, submit a** 500-word abstract describing problem, data, methodology used, and (tentative) results of the research by Dec. 10, 1994 to:
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Seeing voices: A journey into the world of the deaf. By Oliver Sacks. 1989. Berkeley: University of California Press. Pp. 224. $17.95.

Reviewed by Barbara F. Grimes
SIL International Administration

Recognition and development of deaf sign languages began in the eighteenth century with a youthful French abbot, de l'Epée, who wanted to teach the Bible to deaf people. He was inspired by Socrates' remark in the *Cratylus* of Plato, 'If we had neither voice nor tongue, and yet wished to manifest things to one another, should we not, like those which are at present mute, endeavor to signify our meaning by the hands, head, and other parts of the body?'

Sacks says:

> But it is not (usually) the ideas of philosophers that change reality; nor, conversely, is it the practice of ordinary people. What changes history, what kindles revolutions, is the meeting of the two. A lofty mind—that of the Abbé de l'Epée—had to meet a humble usage—the indigenous sign language of the poor deaf who roamed Paris—in order to make possible a momentous transformation. If we ask why this meeting had not occurred before, it has something to do with the vocation of the Abbé, who could not bear to think of the souls of the deaf-mute living and dying unshriven, deprived of the Catechism, the Scriptures, the Word of God; and it is partly owing to his humility—that he *listened* to the deaf—partly to a philosophical and linguistic idea then very much in the air—that of universal language... Thus, de l'Epée approached sign language not with contempt but with awe (p. 16).

The Abbé paid attention to his pupils, acquired their language, and taught them to read; thus 'he opened to them the world's learning and culture'. His school was founded in 1755, and was the first to achieve public support. He trained many teachers for the deaf. By the time he died in 1789, he and his teachers had established 21 schools for the deaf in France and elsewhere in Europe. His book has been translated into many languages.

French Sign Language, developed by de l'Epée and others, is widely acknowledged to be the first deaf sign language that was accepted and developed for education and other purposes. It, in turn, has been taken to many parts of the world, and has influenced the development of other sign languages, including American, which, in its turn, has been taken to many places and has influenced many other sign languages.

Of the 80 or more deaf sign languages we know about, Sacks gives information about several, including American, French, British, Old Kentish, Martha's Vineyard, Danish, Chinese, Australian Aborigines, Yugoslavian, Mayan, Mexican, Providencia, Venezuelan, and Uruguayan.

He mentions cases where large percentages of the population are or were deaf: Martha's Vineyard from 1690 to 1940 (up to 25 percent in some neighborhoods), one village in Mexico which uses Mayan Sign Language (30 percent), and Providencia Sign Language in the Caribbean. In these cases and others, the entire population, including hearing people, has learned and uses the sign language.

Oliver Sacks brings a neurologist's perspective on deafness. He is the author of several books, including the best selling *The man who mistook his wife for a hat*.

Seeing voices is an excellent introduction to the history of deaf sign languages, their development for educational purposes, insights into the relationship of language to thinking and logic, the culture of deaf people, and obstacles the deaf have faced in their struggle to communicate and be accepted.

Sacks highlights several deaf people who did not learn sign language until their adolescent or adult years, because they were not in contact with other deaf people who knew how to sign.

Pierre Desloges in 1779 was one such deaf person. He was the first to publish about his own experience, being deafened at an early age, before he could speak. He eventually was taught sign language by an illiterate deaf-mute person. Later he went to school and learned to write French. He describes his situation before he learned sign language:

> At the beginning of my infirmity, and for as long as I was living apart from other deaf people... I was unaware of sign language. I used only scattered, isolated, and unconnected signs. I did not know the art of combining them to form distinct pictures with which one can represent various ideas, transmit them to one's peers, and converse in logical discourse (p. 18).

Joseph, another deaf person, was eleven before he entered a school for the deaf. He was born deaf, and at first was thought to be retarded, and then autistic. He longed to communicate, but could not.

> Joseph was unable, for example, to communicate how he had spent the weekend—one could not really ask him, even in Sign: he could not even grasp the *idea* of a question, much less formulate an answer. It was not only language that was missing: there was not, it was evident, a clear sense of the past, of 'a day ago' as distinct from 'a year ago'. There was a strange lack of historical sense, the feeling of a life that lacked autobiographical and historical dimension, the feeling of a life that only existed in the moment, in the present... Joseph saw, distinguished, categorized, used; he had no problems with *perceptual* categorization or generalization, but he could not, it seemed, go much beyond this, hold abstract ideas in mind, reflect, play, plan. He seemed completely literal... unable to juggle images or hypotheses or possibilities, unable to enter an imaginative or figurative realm. And yet, one still felt, he was of normal intelligence, despite these manifest limitations of intellectual functioning. It was not that he lacked a mind, but that he was not *using his mind fully* (p. 40).

Theophilus d'Estrella was born deaf and did not start to learn any formal sign language until he was nine. He became a gifted deaf artist and photographer. He wrote the following in 1893:

> I thought in pictures and signs before I came to school. The pictures were not exact in detail, but were general. They were momentary and fleeting in my mind's eye. The [home] signs were not extensive but somewhat conventional [pictorial] after the Mexican style... not at all like the symbols of the deaf and dumb language (p. 41).

After d'Estrella learned sign language, he said it 'served to "elaborate" his thoughts without being necessary for thought in the first place' (p. 41).

British neurologist Hughlings-Jackson says of such persons, they are unable to 'propositionize'. Abbé Roch-Ambroise Sicard wrote that the introduction of signing is as 'opening up the doors of... intelligence for the first time'. Sacks says:

> What emerges from the stories of Joseph and... others like them is a sense of peril—the especial peril that threatens human development, both intellectual and emotional, if the healthy acquisition of language fails to occur. In an extreme case there may be a complete failure in the acquisition of language, complete incomprehension of the idea of language. And language, as Church reminds us, is not just another faculty or skill, it is what makes thought possible, what separates thought from nonthought, what separates the human from the nonhuman (p. 60).

Sign languages can communicate feelings as well as ideas. Desloges writes of sign language:

> The language we use among ourselves, being a faithful image of the object expressed, is singularly appropriate for making our ideas accurate and for extending our comprehension by getting us to form the habit of constant observation and analysis. This language is lively; it portrays sentiment, and develops the imagination. No other language is more appropriate for conveying strong and great emotions (pp. 19-20).

In the last section of the book, Sacks discusses the culture of the deaf. He says:

> Thus, with great rapidity, in the years after 1817 [when the first deaf school in the United States was founded by Clerc and Gallaudet at Hartford in 1817], there spread throughout the States not just a language and a literacy, but a body of shared knowledge, shared beliefs, cherished narratives and images, which soon constituted a rich and distinctive culture. Now, for the first time, there was an 'identity' for the deaf, not merely a personal one, but a social, cultural one. They were no longer just individuals, with an individual's plights or triumphs; they were a *people*, with their own culture, like the Jews or the Welsh (pp. 136-137).

Sacks explains that deaf culture in the United States has been transmitted primarily through the schools for the deaf.

> These schools acted as foci for the deaf community, passing down deaf history and culture from one generation to the next. Their influence went well beyond the classroom: commonly, deaf communities would spring up around the schools, and graduates would often remain close to the school, or even take jobs working in the school. And crucially, most of these schools for the deaf were residential schools, as Carol Padden and Tom Humphries point out: 'The most significant aspect of residential life is the dormitory. In the dormitories, away from the structured control of the classroom, deaf children are introduced to the social life of deaf people' (p. 136).

Probably the first linguist to 'really confront... the reality of Sign' was William Stokoe, who came to Gallaudet University in Washington, D. C., in 1955. In 1960 he published a...

> ...'bombshell' paper on Sign Language Structure, the first-ever serious and scientific attention paid to 'the visual communication system of the American deaf' (p. 140).

In 1965 he published his *Dictionary* of American Sign Language. It was 'the first description of the social and cultural characteristics of deaf people who used American Sign Language' (p. 141).

Other linguists also have now published and continue to publish about other sign languages around the world. It is mainly only those languages which have been described by linguists since 1960 which are known about by the rest of the world.

Finally Sacks describes the demonstrations that took place in 1988 at Gallaudet University, the only liberal arts university for deaf people in the world. There the deaf students demanded and got a deaf president. Sacks concludes his book by asking:

> But has all been changed? Will there be a lasting 'transformation of consciousness'? Will deaf people at Gallaudet, and the deaf community at large, indeed find the opportunities they seek? Will we, the hearing, allow them these opportunities? Allow them to be themselves, a unique culture in our midst, yet admit them as co-equals, to every sphere of activity? One hopes the events at Gallaudet will be but the beginning (p. 158). ■

The Fourth International Symposium on Language and Linguistics
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Bangkok, Thailand - January 8-10, 1996
Institute of Language and Culture for Rural Development
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Interdisciplinary Colloquium Discourse:
Linguistics, Computational, and Philosophical Perspectives
Center for Philosophy of Science, University of Pittsburgh—March 24-26, 1995
Participants: Nicholas Asher, Robert Brandom, Bob Carpenter, Wallace Chafe, Barbara DiEuenio, Daniel Everett, Clark Glymour, Julia Hirschberg, Jerry Hobbs, Hans Kamp, Peter Machamer, Johanna Moore, Megan Moser, Ellen Prince, Lauren Resnick, Michael Rochemont, Candy Sidner, Scott Soames, Mark Steedman, Russ Tomlin, Edwin Williams.
Contact: Dan Everett (dever@pogo.isp.pitt.edu). Deadline for papers to be announced.

Three books on Second Language Acquisition:
English phonetics and phonology: A practical course. 1991. By Peter Roach. Cambridge University Press, 2nd ed. $27.95 including two cassettes. (1984 Student's and teacher's paperback books $12.95 ea. 212 pp.)

Five minute activities: A resource book of short activities.. By Penny Ur and Andrew Wright. 1992. Cambridge handbooks for language teachers. Cambridge University Press. 128 pp.
Hardback $34.95, paperback. $12.95

Tasks for language teachers: A resource book for training and development. 1993. By Martin Parrott. Cambridge teacher training and development. Cambridge University Press.
Hardback $49.95, paperback $24.95.

Reviewed by Genevieve M. Hibbs
Wycliffe Associates, UK

When each of the three books arrived, I mentally responded, 'How useful.' The content is applicable over considerably wider fields than the book designers suggested.

English phonetics and phonology: A practical course. One's introduction to phonetics, the phonetic alphabet and phonemics may be through one's own native language, through a second language, or through languages in the abstract. This systematic and intensive course may be studied with or without a teacher and could be used for a 'stand alone' class for teachers and learners of English or as preparatory to, revision for, or in parallel with a more universal study of the subject. It would be especially useful for non-native English speaking students who must master English as a tool to study a further language or languages.

There is also a case for a teacher using selected material from this text to assist experienced students of English as a Second Language in non-language courses—those who are being held back by not adequately hearing or reproducing particular sounds.

Five minute activities: A resource book of short activities. Although the examples are chosen for English language learning, most can be adapted to requirements for other languages. If it is used in this way the characteristics of the language being learned need to be considered. Also, many of the examples are equally applicable to the study of other disciplines

where words and concepts require comprehension. Some could be used as parlor or party games, and for stimulation in day centers and similar institutions.

The 120 titles include: 'How many things can you think of that...?' 'General knowledge'; 'Likes and dislikes'; 'Songs'. As this book is slim and very light weight, it is likely to accompany me on overseas travels when teaching is on the agenda. Its inexpensive, low tech resources would be invaluable.

Tasks for language teachers: A resource book for training and development. Theory and practical tasks are intertwined in this teacher's course book. It is considerably more academic than the 'Five minute activities...'. The 'other reading' suggestions appear to be valuable, and the notes on those readings would be very useful for people constructing courses who do not have easy access to check them for themselves.

Personally, I found this book a bit didactic in places, and patronizing, though I don't believe this was intentional. The decision to use male and female pronouns in alternating chapters may have answered the author's technical need to avoid sex discrimination, but by specifying *he, him, his* or *she* or *hers* on every possible occasion (nearly) and tending not to use *they, their, them,* for me it reads strongly sexist.

I would disagree with the book designer for heading both the 'task' section and the discussion of the 'task' section with the same typography and two sentences. The fact that 'Task' is white on black and 'Notes' is white on gray did not overcome my feeling of annoyance with it. I felt as if I was on a deliberately confusing mental test, trying to work out what the numbers and alpha characters referred to, especially with the first set.

Many of the tasks are realistic and potentially usable both in the training of teachers and helping them to understand their role, and for lesson resources in the classroom. The text encourages teachers to carry out small scale research in their classrooms and gives good patterns to do so. I would expect to use components of it in a variety of contexts, again, not just for language teaching per se. ■

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Bates, Madeleine and Ralph M. Weischedel, eds. Challenges in natural language processing. 1993. Cambridge, MA: Cambridge University Press. 296 pp. Hardcover $49.95.

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